A Direct Test of Legislative Gatekeeping

Sebastian Thieme*

September 3, 2019

Abstract

Tests of legislative gatekeeping theories have been hampered by the absence of status quo estimates, making these tests vulnerable to selection bias. I overcome this problem with a novel dataset on position-taking by private interests in Iowa, Nebraska, and Wisconsin, because these data record organizations' positions on lobbied bills irrespective of whether the bills receive floor consideration. This permits an estimation of the ideological locations of status quo policies for bills with and without floor consideration, and in turn rigorous empirical tests of agenda control theories. The data provide substantial evidence of gatekeeping, and can adjudicate among specific models of gatekeeping in specific circumstances. In particular, they corroborate partisan gatekeeping in the Iowa House and the Wisconsin Assembly, and cannot distinguish between partisan and nonpartisan accounts in the other chambers. This shows how parties use legislative institutions to control the agenda, and influence the political process in lower chambers.

*Postdoctoral Research Associate, CSDP, Princeton University. sthieme@princeton.edu. Earlier versions of this paper were presented at the 2016 MPSA and the 2017 APSA Annual Meetings, and the NYU Graduate Political Economy Seminar (Spring 2016 and 2017). I gratefully acknowledge valuable advice and suggestions from Sandy Gordon, Pat Egan and Howard Rosenthal. Moreover, I thank Brandice Canes-Wrone, Gleason Judd, Michael Peress, Ellie Powell, Steven Smith, and the participants of the NYU Graduate Seminar for helpful suggestions.

Introduction

Which bills are enacted and which bills never receive consideration on the floor? Committees and other legislative institutions shape the legislative agenda by promoting some bills for consideration and blocking others, thereby helping to determine which bills are ultimately enacted into law. However, it remains an open question how committee members and legislative leaders use their procedural rights to exercise gatekeeping. Does the majority party act as a procedural cartel (Cox and McCubbins 2005, 2007) to deny floor consideration to bills where a vote between a proposal and the status quo policy would find a majority of the majority party on the losing side? Or, do minorities with procedural rights act efficiently and without a partisan lens to screen out those bills that are unlikely to find the support of institutional veto players (Crombez, Groseclose, and Krehbiel 2005; Krehbiel 1998, Ch. 10)?

Which bills receive floor consideration has important consequences not only for which bills can pass, but also which legislative coalitions can form (Snyder 1992a), and which public policies legislators are expected to take positions on. From a normative perspective, gatekeeping that anticipates the preferences of institutional veto players would be evidence of an efficient distribution of work within the legislature (Crombez, Groseclose, and Krehbiel 2005), notwithstanding different potential normative evaluations of the policy gridlock – or stability – induced by those veto players.

At the same time, partisan gatekeeping may be desirable if responsible parties serve as an integrating mechanism and a necessary requirement for representative democracy (American Political Science Association 1950; Bryce 1888; Cox and McCubbins 2005, 229; Schattschneider 1942). Alternatively, "responsible-party government" may be viewed as problematic if legislators are expected to represent their local constituents (e.g., Miller and Stokes 1963). Moreover, to the extent

that partisan gatekeeping marginalizes moderate legislators (Cox and McCubbins 2005, 31), it may contribute to a "democratic deficit" in that enacted bills implement policies that are too extreme for voters (Lax and Phillips 2012, 164). These questions are especially important due to the recent increase in the number of state legislatures that are controlled by one party (Badger, Bui, and Pearce 2018).

Previous empirical tests of gatekeeping have lacked information about status quo locations and proposals, especially for bills that never receive a vote. This absence has hindered a strong empirical validation of pivotal politics theories (Clarke, Gray, and Lowande 2018), as well as other theories of lawmaking. Selection bias in prior tests arose not only from the absence of pre-floor measures, but also a conflation of positive and negative agenda control (Jenkins and Monroe 2016). Further, extant analyses using estimates from ideal point models and roll call votes to test theories of lawmaking have been subject to the critique that the estimates generated by an endogenous agenda conforming to either of several theories (e.g., the pivotal politics or party cartel theories) may be too imprecisely estimated to distinguish between them (Clinton 2007).

In this paper, I use both floor and pre-floor data from the state legislatures of Iowa, Nebraska, and Wisconsin for a direct test of gatekeeping. These states' legislatures require or permit lobbyists to declare the position they are communicating towards legislators on behalf of their principals at different times in the legislative process. Treating private interests' positions as final passage votes on bill versions permits estimating the ideological positions of private interests and legislators in the same space, using a Bayesian item-response model (Thieme Forthcoming).

More importantly for the current application, group positions on bills that receive floor consideration and bills that do not, allow me to estimate the spatial locations of status quos associated with both sets of bills. This provides a data-driven approach to addressing the endogeneity of the

agenda, and permits a direct measure of gatekeeping which is not confounded by conflating negative and positive agenda control: the proportion of blocked bills out of the bills that are predicted to be blocked by a particular gatekeeping hypothesis. I compare this ratio to a plausible null hypothesis of "random gatekeeping" for a direct test of nonrandom gatekeeping hypotheses. Further, I compare the performance of partisan and the nonpartisan hypotheses against each other.

The analysis provides substantial evidence of nonrandom gatekeeping and, unsurprisingly, shows that many bills are introduced even though they are likely to blocked, suggesting either uncertainty or pure position-taking. Furthermore, comparing the distributions of status quo positions, I find evidence that favors partisan gatekeeping in some chambers (the Iowa House and the Wisconsin Assembly), but cannot distinguish between partisan and nonpartisan hypotheses in others (the Iowa Senate, the Nebraska Legislature, and the Wisconsin Senate). The results show how parties use partisan legislative institutions to exercise negative agenda control, and influence the political process in lower chambers. Furthermore, I conduct a direct examination of the pivotal politics theory's main prediction that status quos located within the gridlock interval will not move (Krehbiel 1998). I find supportive evidence for this prediction in Iowa and Nebraska, but not in Wisconsin.

Background

The research on legislative agenda control is part of a broader set of questions about the role of parties in lawmaking in American politics. On the one side are accounts by *partisan* theories in which majority party leadership, especially in the U.S. House of Representatives, affects legislative outcomes either by applying pressure to ensure party-line voting (Aldrich 1995) or by acting

as a procedural cartel (Cox and McCubbins 2005, 2007).[1] In the latter theory's view, the majority party's agents exercise negative agenda control mainly via *gatekeeping*, especially through the pre-floor screening of bills in committees, to keep bills off the floor agenda which would be opposed by a majority of members of their party.[2] More precisely, the party cartel theory predicts that bills with status quos in the *majority party blockout zone* should not receive consideration on the floor of the chamber (Cox and McCubbins 2005, 43).[3]

On the other side, the nonpartisan pivotal politics theory (Krehbiel 1998) emphasizes the primitive preferences of legislators and institutional veto players.[4] The theory predicts no policy change for bills with status quo policies in the gridlock interval.[5] While the pivotal politics theory does not explicitly incorporate gatekeeping, Crombez, Groseclose, and Krehbiel (2005) suggest that ostensible gatekeeping is consistent with committees or members with procedural rights anticipating a gridlock interval.[6] In the main analysis, I therefore test the pivotal politics-consistent hypothesis that bills with status quos in the gridlock interval will be denied floor consideration. In a supplementary analysis, I return to the explicit prediction of the pivotal politics model by examining the extent to which bills with status quos in the gridlock interval are enacted. Figure 1 illustrates each theory's censored interval, as well as the predicted outcome based on the status quo and the preferences of key actors.

[1] Krehbiel, Meirowitz, and Wiseman (2015) develop a theory that includes majority and minority party influence.

[2] Positive agenda control has been defined as 'the ability to push bills through the legislative process to a final-passage vote on the floor' (Cox and McCubbins 2005, 20). In this paper, I concentrate on the party cartel theory, as it emphasizes the importance of gatekeeping.

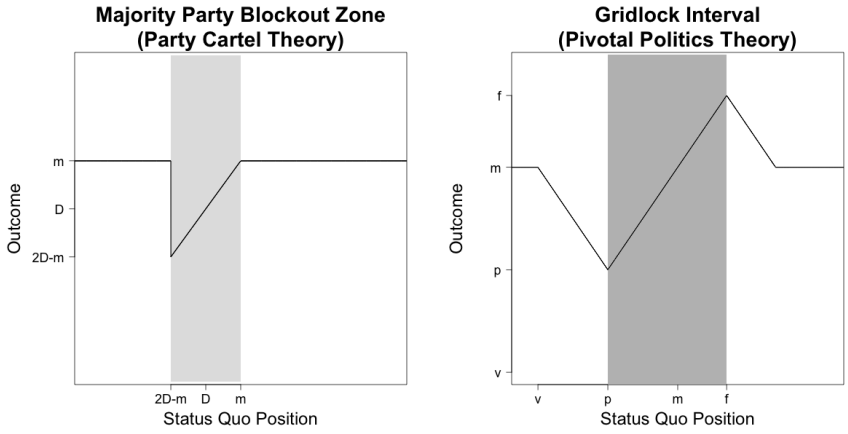
[3] The majority party blockout zone extends from the floor median to a position that has the same distance to the majority party median as the floor median, but is on the other side of the majority party median.

[4] Examples of such institutional veto players are the filibuster pivot, the president, and the veto override pivot. See Brady and Volden (2006) for a similar argument. For examples of hybrid pivot-and-party models, see Chiou and Rothenberg (2003), Cox and McCubbins (2005, 177-181), or Peress (2013).

[5] Krehbiel (1998, 35) defines gridlock as the absence of policy change despite the existence of a majority that favors a change. The gridlock interval is determined by the configuration of the pivotal actors.

[6] This is also implied by Krebiehl (1998, Ch. 10). Kypriotis' (2013) strong majoritarian version of the Committee Bill Reporting model assumes that committees will act as if they shared the preferences of the chamber median.

Figure 1: The Party Cartel and the Pivotal Politics Theory



Note: The panels show bill outcomes predicted by the party cartel (left) and the pivotal politics theory (right) as a function of the status quo and the preferences of key actors. They also show the intervals for which no policy change is expected. The party cartel theory predicts that bills with status quos in the majority party blockout zone will not receive floor consideration. The blockout zone [2D-m, m] is determined by the chamber median (m) and the majority party median (D). The example gridlock interval [p, f] is determined by the presidential pivot (p) and the filibuster pivot (f) (see Krehbiel 1996, 22). Since bills with status quos in the gridlock interval are predicted not to pass, a pivotal-politics consistent gatekeeping hypothesis is that members with procedural rights will keep such bills from receiving floor consideration.

Previous empirical tests of gatekeeping have been limited by the absence of direct estimates about bill proposal and status quo locations (Clarke, Gray, and Lowande 2018), especially for bills that never receive floor consideration.[7] Instead, many studies (e.g., Cox and McCubbins 2005; Gailmard and Jenkins 2007; Cox, Kousser, and McCubbins 2010; Jackman 2014) have tested theories of negative agenda control using floor-based measures, such as the majority and minority party roll counts or roll rates.[8] The results from these studies have tended to find evidence supporting the party cartel theory, both in Congress and in state legislatures.[9]

[7]Peress (2013), Richman (2011), and Woon and Cook (2015) are partial exceptions to this. Peress (2013) directly estimates status quos for bills that receive floor consideration (see below). Woon and Cook (2015) use structural models that account for temporal dependence to generate probability distributions of status quo policies. Richman (2011) directly estimates status quos for broad policy areas, but does not examine gatekeeping on individual bills. Richman (2011) and Woon and Cook (2015) find that a combination of pivots and party best explains the data.

[8]The majority party is rolled if a bill or motion passes against the opposition of a majority of its members. The roll rate is defined as the ratio of minority to majority party rolls. Low majority party roll rates are taken as evidence in favor of partisan gatekeeping.

[9]For example, Cox and McCubbins (2005, Ch. 5) find low majority party roll rates and relatively higher minority party roll rates in the House, and somewhat higher majority roll rates in the Senate. Further, Gailmard and Jenkins

However, using low majority roll rates as evidence for partisan gatekeeping is problematic for several reasons. First, roll rates, i.e., the ratio of minority to majority party rolls, conflate negative and positive agenda power by including minority rolls (Jenkins and Monroe 2016). Second, a credible test of partisan gatekeeping requires a baseline roll rate that could occur with behaviorally inconsequential parties (Krehbiel 2007). Third, special rules that reduce debate or the ability to offer amendments further limit the usefulness of roll call analyses (Schickler and Pearson 2009).

A more general problem with floor actions is that they can only reveal the "fingerprints" of the actual level of negative agenda control, because effective negative agenda control would imply that agents of the majority party are able to veto legislation at the pre-floor stage (Gailmard and Jenkins 2007; Jenkins and Monroe 2016). A convincing test between competing hypotheses should include not only majority party rolls, but also successful blocks by the majority party (Jenkins and Monroe 2016; Krehbiel, Meirowitz, and Woon 2005). To illustrate this point, consider two different legislatures, each with two majority party rolls, but with a different number of blocked bills. In the first legislature, two bills that would have rolled the majority are blocked in committee. In the second case, eighteen such bills are blocked. Arguably, the degree of negative agenda control is higher when committees block 90% rather than 50% of potential majority party rolls.[10]

A second line of research on negative agenda control has employed cutpoints of final passage votes estimated using item-response models or W-NOMINATE scaling (Poole *et al.* 2011), result-

(2007) show that when excluding nominations, the Senate shows similar majority roll rates to the House. Cox, Kousser and McCubbins (2010) investigate the effects of institutional changes on the majority and minority roll rates, as well as the direction of policy change, by exploiting two quasi-experiments in California and Colorado. Moreover, Anzia and Jackman (2013) find that "gatekeeping institutions" – the ability of majority-appointed committees to deny a hearing and the option not to report bills out of committee – reduce majority party roll rates in state legislatures. Using similar data, Jackman (2014) finds that this is mitigated by majoritarian rules.

[10] Of course, this example only considers bills that are introduced. A measure of gatekeeping that would incorporate bills that are never introduced would have to rely on assumptions about when a proposal is made, or a credible source of plausible bill proposals.

ing in mixed findings with respect to the party cartel theory (Krehbiel, Meirowitz, and Woon 2005; Clinton 2007; Stiglitz and Weingast 2010). Krehbiel, Meirowitz, and Woon (2005) derive a set of intervals for which observed cutpoints in the U.S. Senate are inconsistent with either theory and compare the percentage of final passage votes with cutpoints in those intervals. However, based on the differing sizes of the censored intervals, the authors argue that the theories are faced with varying levels of exposure to falsification, which is a version of the "fingerprints" critique mentioned above. To adjust for differential exposure, they assume an "atheoretic" normal distribution of cutpoints as a null hypothesis.[11] A different critique of cutpoint analyses is that the ideal point estimates generated by an endogenous agenda conforming to either the pivotal politics or party cartel theory may be too imprecisely estimated to distinguish between them (Clinton 2007). This critique is based on the argument that cutpoints would be missing for the censored intervals. Hirsch (2011), Clinton (2012), and Krehbiel and Peskowitz (2015) show that whether this is a problem depends on the amount of error that is present in roll call voting.

A third approach is to directly estimate ideal points, proposal locations and status quo positions on the same scale via a statistical model that combines roll call votes with cosponsorship data (Peress 2013). Here, bill positions are estimated at final passage – or functionally equivalent – votes via the cosponsors of the originally introduced bill (unamended bills), or via the cosponsors of the last successful amendment (amended bills). Status quo positions are then calculated as the reflection of the proposal's position on the estimated cutpoint. The results from Peress's (2013) analysis of bills in the U.S. Senate support the hypothesis of partisan gatekeeping. However, the analysis is limited to bills that receive a final passage vote, and does not examine what types of

[11] Adjusted for differential exposure, Krehbiel, Meirowitz, and Woon (2005) find that the pivot theory does slightly better than the cartel theory. Clinton (2007) conducts a similar analysis for the House and Senate and finds little evidence for either theory. Stiglitz and Weingast (2010) analyze the U.S. House using both the distribution of cutpoints and the uncertainty of estimates in specific intervals.

bills are blocked.

A Direct Test of Gatekeeping

I develop a direct test of gatekeeping that leverages mandatory state-level disclosure by lobbying principals, as well as legislators' votes and cosponsorship decisions to address the challenges faced by previous tests of negative agenda control. In particular, the positions by lobbying principals on bills that are reported out of committee and bills that die in committee allow me to estimate status quos for both sets of bills. Moreover, data on bill cosponsorship decisions reveal additional information about the positions of bill proposals (e.g., Peress 2013; Woon 2008). I combine positions of lobbying principals with legislative roll call votes and bill cosponsorship decisions in a joint statistical model of bill cosponsorship and voting to directly estimate the status quos (Peress 2013).

This approach permits a more direct measure of gatekeeping, the number of bills that are actually blocked as a proportion of the number of bills that are predicted to be blocked by a specific gatekeeping hypothesis. Unlike roll rates, this *gatekeeping ratio* is not confounded by including positive agenda control (Jenkins and Monroe 2016). Further, it eliminates the need to adjust the test with an assumed exposure to bill cutpoints (Krehbiel, Meirowitz, and Woon 2005). In addition, by jointly scaling lobbying principals and legislators, the principals' positions on bills that are kept off the agenda can reduce the danger that the estimates will suffer from imprecision due to an endogenous agenda. The estimates therefore enable a data-driven approach to addressing a prominent critique of using ideal point estimates and cutpoints to test theories of lawmaking (Clinton 2007, 2012; Hirsch 2011; Krehbiel and Peskowitz 2015).

I compare the gatekeeping ratio to a plausible null hypothesis of "random gatekeeping" – the

proportion of bills with status quo estimates that are blocked. Since status quos are estimated in a Bayesian MCMC model, I employ credible sets (Thulin 2014) of the difference between predicted and actual proportions for direct tests of the gatekeeping hypotheses. Further, I compare the relative performance of the partisan and nonpartisan hypotheses.

Data

Lobbyist Declarations in the Iowa, Nebraska, and Wisconsin state legislatures provide a novel source of position-based data for estimating private interests' and legislators' positions in a common space (Thieme Forthcoming). Previous studies have used interest group positions from legislator ratings to estimate the ideology of legislators and these organizations on the same scale (Gerber and Lewis 2004; Poole and Rosenthal 2007). However, not only are these ratings relatively rare, but also voluntary disclosure of positions may induce substantial bias, including artificial extremism (Snyder 1992b).

Although all 50 states have some reporting requirements for lobbyists who lobby their legislatures, disclosure requirements vary substantially by state.[12] Current lobbying rules in Iowa, Nebraska, and Wisconsin are unusual in that they require lobbyists to report the bills on which they lobby legislators, as well as the principal on whose behalf they lobby on each bill. Crucially, they also require or permit lobbyists to declare their principals' positions on lobbied bills. Nevertheless, there are several differences in the reporting requirements. First, lobbyists in Iowa and Nebraska are required to report their principals' positions, while lobbyists in Wisconsin may leave the position undisclosed.[13] Second, the states differ in how quickly lobbyists have to report lobby-

[12]For an overview from the National Conference of State Legislatures, see http://www.ncsl.org/research/ethics/50-state-chart-lobbyist-registration-requirements.aspx.

[13]Between 2003 and 2016, only 16.7% of positions were not disclosed. To address this and to augment the data, I collected positions registered by legislators and lobbying principals at committee hearings in Wisconsin (2003-2016).

ing activity and in their options for reporting positions. Finally, the states differ in how far back in time data is made available online.[14]

I assembled a dataset of all lobbyist declarations from Iowa, Nebraska, and Wisconsin between 2003 and 2016.[15] For all three states, I further collected roll call data, bill histories, committee leadership data, and bill (co-)sponsorship data from 2003 through 2016. In addition, I collected bill cosponsorship data to construct a matrix of legislators' cosponsorship decisions. For a supplementary analysis, I also collected data on the sponsor of each bill. To determine the positions of Iowan and Wisconsinite governors on bills, I employed bill histories and cosponsorship data.[16] In particular, I used bill sponsorship data and bill histories to find bills that were introduced on behalf of the governor. Moreover, I employed the bill histories to find bills that were signed into law (without partial veto), which should reduce the potential for artificially extreme ideal point estimates of the executive (see Treier 2010). Finally, I used the bill histories to find bills that received a full veto from the governor. To find information about party affiliation in Nebraska, I consulted the biannual Blue Books.

Combining Position-Based Data from Multiple Sources

I use the lobbyist declaration data from each state to construct three separate vote matrices that combine principals' positions with roll call votes in the Iowa, Nebraska, and Wisconsin state legislatures. For each state's legislature, I employ the bill histories to find the dates of successful amendments so as to determine which bill version was current at a particular date. I assume that any amendment approved by a floor vote constitutes a change in the version of a bill. Further, I

[14] Appendix A provides more details on the reporting requirements in the three states.

[15] Since lobbyist declarations in Iowa before 2005 did not include the lobbyists' principals, these declarations are not used in the analysis.

[16] In Iowa, lobbyist registrations are also made on behalf of state agencies and the Governor's Office.

assume that any declaration applies to the then-current bill version, and not to previous versions. I then assemble the vote matrix by combining the principals' positions on bill versions with roll call votes via bill versions whenever a bill version is associated with a final passage vote. When bill versions are not associated with a roll call vote – for example, because a bill died in committee – I add the positions associated with the bill version to the matrix as a separate column.[17]

Estimating Status Quo Positions

To estimate the status quo positions of proposed bills, I combine data from roll call votes, lobbying declarations, bill cosponsorships, and legislator covariates to estimate a joint model of voting and bill cosponsorship, based on the model developed by Peress (2013). The first part of the model consist of a standard item-response model, based on a spatial model of voting (e.g., Bafumi *et al*. 2005; Clinton, Jackman, and Rivers 2004). It relies on a vote matrix to estimate the cutpoint and discrimination parameters of votes, as well as the ideal points of elected officials and lobbying principals.[18] The second part of the model uses bill cosponsorship decisions, bill-fixed effects, as well as legislator covariates on party affiliation and committee leadership to estimate the positions of bill proposals on the same ideological dimension as that recovered from the voting model.[19]

Based on the assumption of symmetric preferences, the status quo can be calculated as the reflection of the proposal location on the estimated cutpoint, i.e. $\hat{s}_t = 2\hat{\tau}_t - \hat{p}_t$.[20] This approach provides an alternative to the estimation of status quos via the voting data alone, and does not rely

[17] Appendix B.1 provides further details on how votes and positions from declarations are combined via bill versions. Appendix B.2 describes how lobbyist declarations and positions from committee registrations are combined with roll call votes via bill versions. Appendix B.3 describes the procedure for merging or splitting the position-record of principals across sessions of the same state.

[18] For bills without floor consideration, the estimates are based on lobbying declarations. For bills with floor consideration, the estimated are based on roll call votes and lobbying declarations.

[19] Following Peress (2013, 619), I restrict attention to bills with at least 3 cosponsors.

[20] Here, $\hat{s}_t$, $\hat{\tau}_t$ and $\hat{p}_t$ are the status quo, cutpoint and proposal location estimates for bill t.

on assumptions about bill-specific errors for identification (Peress 2013).[21] However, since many bills lack cosponsors it also limits the number of bills for which status quos can be estimated with sufficient accuracy (Peress 2013, 619).[22]

To obtain status quo estimates for a larger set of bills, I employ adjusted vote-based status quo estimates when a lack of cosponsorships prevents a sufficiently accurate estimate of p_t. In particular, instead of relying on the strong assumption of a homogenous bill-specific error standard deviation (σ), I adjust vote-based status quos using estimates of bill-group specific σ_g.[23] This approach increases the number of estimates from 79 to 760 (Iowa House), from 45 to 628 (Iowa Senate), from 135 to 531 (Nebraska), from 534 to 614 (Wisconsin Assembly), and from 388 to 501 (Wisconsin Senate).[24]

The joint model differs from Peress (2013) in several ways. First, the voting model is parameterized to estimate a cutpoint parameter τ_t. Moreover, the two parts of the model only share information about the ideal point parameters and not also about bill positions and status quos. This allows me to compare status quos identified via cosponsorships and vote choice with the status quo estimates based only on vote choices.[25] Finally, I use a logistic, instead of a normal distribution function to model the error terms i.e., $F = logit^{-1}$. I implement the model using the programming language Stan (Carpenter *et al*. 2017) via the R package *rstan* (Stan Development Team 2017).[26]

[21]NOMINATE models identify the proposal and status quo location from the data via the choice of a scaling parameter and the non-linearity of the choice function (see Carroll *et al*. 2009, 567).

[22]This is especially limiting in Iowa, where most bills are introduced by committees and only about 26% of proposals between 2003 and 2016 had at least one cosponsor, compared with approx. 78% in Wisconsin.

[23]The groups are based on chamber, session, and majority party status. See Appendix C.1 for more details.

[24]In the sample, the number of bills with floor consideration is 325 (43%), 325 (52%) 71 (13%), 321 (52%), and 253 (51%), respectively.

[25]Before presenting the main results, I compare the cosponsorship-based status quos with the adjusted vote-based status quos. In Appendix D, I also compare cosponsorship-based status quo estimates with vote-based status quos assuming homoskedasticity ($\sigma_t = 1$, for all t), and with status quos based on assumptions about the proposal location. Moreover, Appendices F.1 and F.2 present results from a series of alternative estimates. For each set of estimates, the results are substantially very similar to the main results.

[26]See Appendix C.2 for more details about the estimation.

Adapting Gatekeeping Hypotheses to State Legislatures

Since the party cartel and pivotal politics gatekeeping hypotheses are derived from theories about lawmaking in Congress, it is worth discussing to what extent the hypotheses should apply to state legislatures with a different set of rules and institutions, and whether the predictions need to be modified to be applicable. In Iowa and Wisconsin, chamber rules put majority party legislators in positions that enable them to block legislation, in a way that is largely consistent with the party cartel theory (Cox and McCubbins 2005, Chapter 3).[27] In particular, agents of the majority party are in the position to assign or reassigning the chairs of committees. Further, committees in Iowa and Wisconsin have the ability to deny a bill a hearing and are not required to report all bills to the floor (Anzia and Jackman 2013).[28] Moreover, agents of the majority party have procedural rights to control the floor agenda.[29]

The nonpartisan Nebraskan legislature provides a difficult test for the partisan gatekeeping hypothesis, since its rules do not recognize a majority party, and since senators elect the Speaker and committee chairs via secret ballots.[30] Further, its rules require a public hearing for almost all bills, and facilitate withdrawing bills from committees (Rule 3, Sec. 13 (b), Rule 5, Sec. 12 and Rule 3, Sec. 14). Although Democrats are frequently elected to chair committees and may hold a majority of committee seats (Schaffner 2007, 483), all sessions in the sample had a Republican majority and elected a Republican as Speaker. Therefore, I test the partisan gatekeeping hypothesis by calculating blockout zones using the median Republican senator.

[27] In 2005-2006, the Iowa Senate was split 25-25 and each committee had a Democratic and a Republican co-chair. Here, I adapt the party cartel theory's prediction by using the union of the two blockout zones.

[28] Anzia and Jackman (2013) call these procedural rights *nonhearing* and *nonreporting* rights.

[29] See Appendix E.1 for details about majority party agenda control in Iowa and Wisconsin.

[30] See Rule 1, Sec. 1 (a) and Rule 3, Sec. 8 (a). In addition, the Nebraska Constitution (III-4) requires senators to be elected on a nonpartisan ballot.

Differences in the cloture requirements between Congress and the three state legislatures make it necessary to adjust the pivotal politics gatekeeping hypothesis. Unlike the U.S. Senate, the Iowa and Wisconsin legislatures only require a simple majority – not a supermajority – for a cloture vote to shut down a filibuster.[31] Hence, the *filibuster* pivot in these states has to be replaced by one of the chamber medians. Since Iowa and Wisconsin both require two-thirds of elected members in each chamber to override a gubernatorial veto[32] the veto override pivot needs no adjustment. Therefore, the gridlock intervals in Iowa and Wisconsin are given by the following lower and upper pivots:

$$p_l = \begin{cases} \min(m_1, m_2) & \text{if } g \geq \max(m_1, m_2) \text{ or } m_1 > g > m_2 \text{ or } m_1 < g < m_2 \\ \max(g, \min(v_1, v_2)) & \text{if } g \leq \min(m_1, m_2) \end{cases} \tag{1}$$

$$p_u = \begin{cases} \min(g, \max(v_1, v_2)) & \text{if } g \geq \max(m_1, m_2) \\ \max(m_1, m_2) & \text{if } g \leq \min(m_1, m_2) \text{ or } m_1 > g > m_2 \text{ or } m_1 < g < m_2, \end{cases} \tag{2}$$

where m_1 is the lower chamber median, m_2 is the upper chamber median, g the governor, and v_1 and v_2 are the two-thirds veto override pivots in the respective chambers.

In Nebraska, 33 out of 49 senators are required to invoke cloture (Rule 7, Sec. 10).[33] Since a gubernatorial veto override requires only 30 votes (Nebraska Const. Art. IV, Sec. 15 and Art. IV, Sec. 7), the filibuster pivots on either side of the median define the gridlock interval. Ordered from most liberal to most conservative, these are senators 17 and 33.

[31] While "fiscal" bills require a quorum of two-thirds of elected members (Wisc. Const. Art. VIII, Sec. 8), "fiscal bill" has been narrowly defined (Annotated Wisconsin Const., Art. VIII, Sec. 8, 60 Atty. Gen. 245).

[32] See Iowa Const. Art. III, Sec. 16 and Wisc. Const. Art. V, Sec. 10.

[33] See Appendix E.2 for more details about the filibuster in Nebraska.

A Direct Measure and Test of Gatekeeping

To assess the relative performance of each theory's gatekeeping hypothesis, I use the following *gatekeeping ratio* (*GR*). It measures the proportion of bills with status quos in a particular theory's censored interval that are correctly predicted to receive no floor consideration. Formally,

$$GR = \frac{N_{b,pb}}{N_{b,pb} + N_{\neg b,pb}}, \tag{3}$$

where $N_{b,pb}$, is the number of bills that are blocked and are predicted to be blocked and $N_{\neg b,pb}$ is the number of bills in the censored interval that receive floor consideration (are not blocked). This measure incorporates the insight that a convincing test between competing theories of negative agenda control should include not just failures to block, but also successful blocks of bills that are predicted to be blocked (Krehbiel, Meirowitz, and Woon 2005; Jenkins and Monroe 2016). A higher gatekeeping ratio indicates a better relative performance for the hypothesis associated with the predictions. For simplicity, I calculate the measure only based on decisions in the chamber where a bill was initially introduced.[34]

Gatekeeping ratios are sensitive to the kind of original bill versions for which cutpoints can be estimated, as well as the relative frequency of estimates for blocked and reported bills. These depend on features of the data generating process, such as the number of principals' positions on a bill, whether or not lobbying was two-sided, lobbyists' reporting requirements, and whether bills were amended before passage. They also depend on minimum vote and lopsidedness requirements

[34]Further, I exclude estimates of votes that are not sufficiently correlated with the estimated dimension. I also exclude duplicate estimates due to multiple votes on the original bill version. In Iowa and Wisconsin, I exclude resolutions if they do not require the approval of the governor. In Nebraska, I exclude votes on appropriations tied to regular bills to avoid duplication. Additional details are available upon request.

that are applied to the vote matrix.[35] This implies that value of the gatekeeping ratio does not have a straightforward interpretation by itself.

However, the relative value of a gatekeeping ratio can be used for a direct test of the different gatekeeping hypotheses. In particular, for each hypothesis and set of estimates I compare the gatekeeping ratio to the plausible null of random gatekeeping, which is given by the overall proportion of estimated bills that are blocked by committee. To characterize the model's uncertainty about the gatekeeping ratios, I calculate the ratios for each of the samples from the posterior distribution and aggregate the ratios across samples. For each alternative hypothesis, I reject the null if the 95%-credible intervals of the difference between the gatekeeping ratio and the proportion associated with random gatekeeping do not include zero.[36] Similarly, I rely on 95%-credible intervals to test the relative performance of the nonrandom gatekeeping hypotheses.

Results

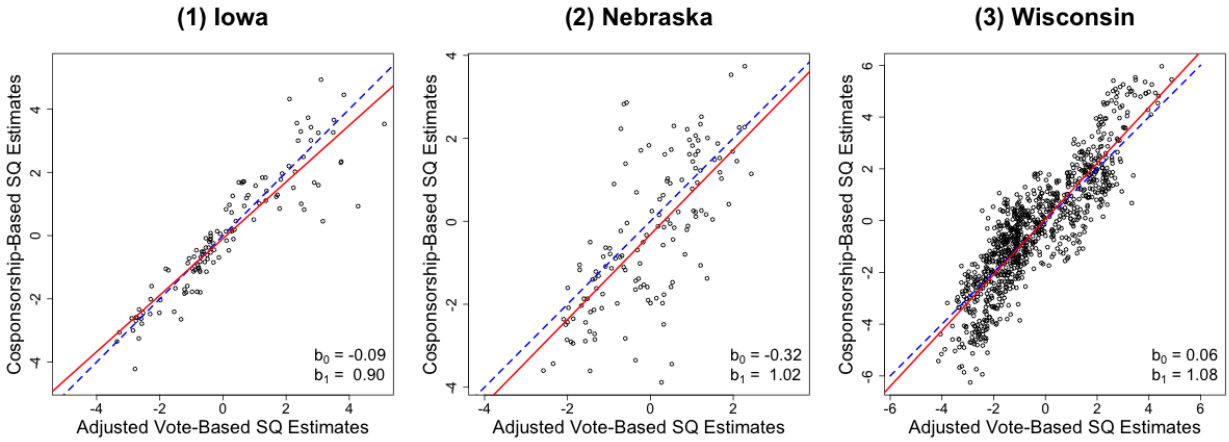
Before presenting the main results, I show that the adjusted status quo estimates can provide an accurate approximation of the status quo estimates based on cosponsorship decisions. In Figure 2, I report the adjusted estimates against the cosponsorship-based estimates for Iowa, Nebraska, and Wisconsin. In each state, there is a strong correlation between the two sets of estimates (0.91, 0.70, and 0.87). Further, the regression lines generally track the 45-degree line closely, particularly in the ranges with the highest density of status quo estimates. This suggests that adjustments of vote-based status quos estimates via estimates of the heterogeneous error variance can be a convenient shortcut to estimating status quos via cosponsorship. Employing the adjusted status quos instead

[35] See Appendix B.1 for details about the lobbying disclosures that affect the data generating process and Appendix C.2 for details about the minimum vote and lopsidedness requirements.

[36] See Thulin (2014) for a decision-theoretic justification of testing a precise null using credible sets.

of the cosponsorship-based status quos increases the number of estimates from 1181 to 3034, an increase of more than 150%. In Iowa and Nebraska, the increases – from 124 to 1388 and from 135 to 531 – are particularly high.[37]

Figure 2: Adjusted Status Quo Estimates Versus Cosponsorship-Based Status Quo Estimates



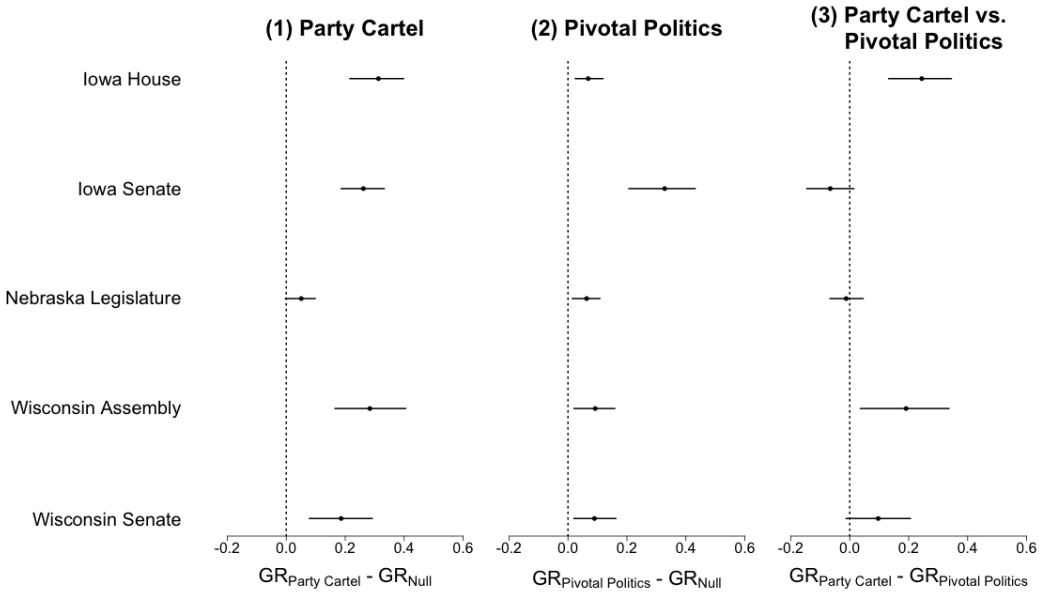
Note: The three panels compare vote-based status quo estimates that are adjusted for potential heteroskedasticy in σ by state, chamber, session, and majority-party status (horizontal axis) to status quos that are estimated using bill cosponsorship decisions and votes (vertical axis). The dashed blue lines represent 45-degree lines, and the solid red lines represent the respective regression lines. The regression coefficients are shown in the bottom right corner of each panel. The number of comparisons is 124 in Iowa, 135 in Nebraska, and 922 in Wisconsin.

Tests of Partisan and Nonpartisan Gatekeeping Hypotheses

In Figure 3, I show the results from a series of direct tests of nonrandom gatekeeping in Iowa, Nebraska, and Wisconsin. For each legislative chamber, I present the results from three hypothesis tests. The results in the first two panels examine whether the null hypothesis of random gatekeeping can be rejected in favor of the partisan party cartel or the nonpartisan pivotal politics gatekeeping hypotheses. The third panel tests the alternative hypothesis that there is a difference between the

[37] Appendices F.1 and F.2 present results using unadjusted vote-based status quo estimates, estimates that rely on bill cosponsorships, and estimates that rely on assumed bill positions. For each set of estimates, the results are substantially very similar to the main results.

Figure 3: Tests of Gatekeeping Hypotheses



Note: The panels in this figure present hypothesis tests of nonrandom gatekeeping using the 95%-credible interval of the difference between two proportions. The first panel tests the party cartel hypothesis against a plausible null hypothesis: the proportion of estimated bills that are blocked. The second panel shows tests of the pivotal politics gatekeeping hypothesis against the plausible null. The third panel examines whether there is a difference between the performance of the party cartel and the pivotal politics gatekeeping hypotheses.

performance of the party cartel and the pivotal politics gatekeeping hypotheses.

The first panel shows that the null hypothesis is rejected in favor of the party cartel hypothesis in both chambers of the Iowan and Wisconsinite legislatures. However, the null of random gatekeeping cannot be rejected in favor of the party cartel hypothesis in the Nebraska Unicameral. Further, based on the results in the second panel, the null hypothesis can be rejected in favor of the pivotal politics gatekeeping hypothesis in all five chambers.

In a direct comparison of the party cartel and pivotal politics hypotheses (Panel 3), the party cartel hypothesis performs better in two out of five chambers. In particular, based on the 95%-credible intervals we can reject the null of no difference between the gatekeeping ratios in the Iowa House and the Wisconsin Assembly. Due to model uncertainty, the tests cannot distinguish

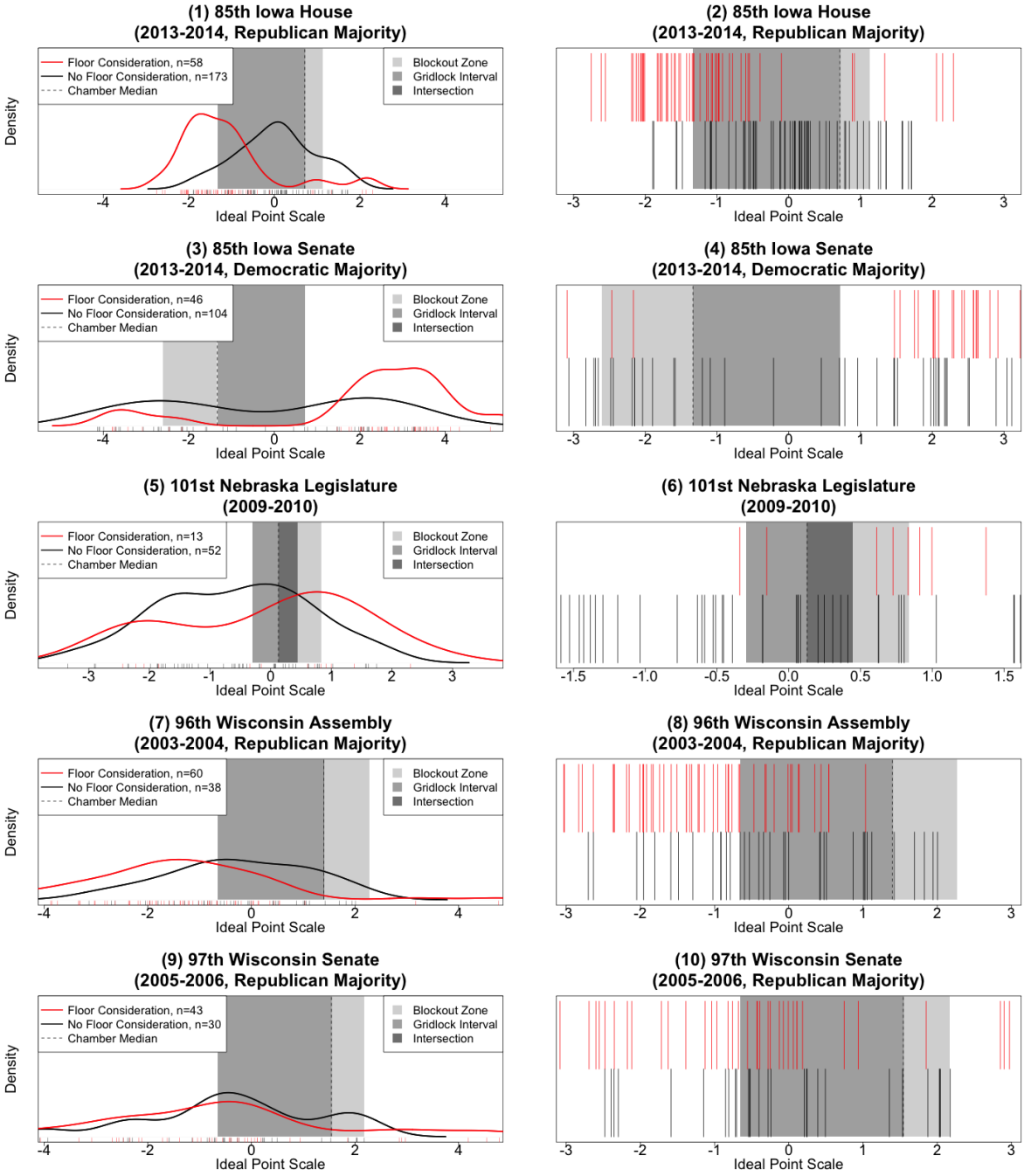
between the party cartel and the pivotal politics gatekeeping hypotheses in the Iowa Senate, the Nebraska legislature, and the Wisconsin Senate.

Figure 4 presents status quo estimates from legislatures and sessions that are most representative of these overall results. The panels show status quo estimates of bills introduced in the 85th Iowa House, the 85th Iowa Senate, the 101st Nebraska legislature, the 96th Wisconsin Assembly, and the 97th Wisconsin Senate (Panels 9 and 10). Status quos for bills that receive floor consideration are shown in red, whereas the status quos of bills that do not receive floor consideration are shown in black.

Unsurprisingly, most bills that receive floor consideration are shown to have associated status quos that fall on the minority side of the ideological spectrum. In particular, chambers with a Republican tend to give floor consideration to bills with a "left" or "liberal" status quo, and chambers with a Democratic majority tend to give floor consideration to bills with a "right" or "conservative" status quo. This pattern, which is prominent in Iowa and Wisconsin, is consistent with the "second commandment" of party leadership described by Cox and McCubbins (2005, 24) as "[t]hou shalt aid bills that most in thy party like" which is meant to promote a record of legislative accomplishment and strengthen the party brand. It is also consistent with an ideological argument for party labels in which a higher variance of the party member's positions is harmful to the party brand and therefore harmful to the electoral chances of the party's candidates (Grynaviski 2010).

The pattern is not found in the Nebraska legislature, where the distribution of status quos for bills that receive floor consideration is more equally distributed and tends to fall outside of both censored intervals. Reflecting the main results, the proportion of bills that receive floor consideration is higher in the gridlock interval than the blockout zone in the Iowa House and both chambers of the Wisconsin legislature, about the same in the Nebraska legislature, and somewhat lower in

Figure 4: Estimated Status Quo Positions



Note: The panels in this figure show status quo estimates of bills introduced in the Iowa House (2013-2014), Iowa Senate (2013-2014), Nebraska legislature (2009-2010), Wisconsin Assembly (2003-2004), and Wisconsin Senate (2005-2006). The panels in the left column displays the distribution of estimates, while the right column focuses on the estimates that lie in the censored intervals. Status quo estimates of bills for bills that receive floor consideration are shown in red, whereas those that do not receive floor consideration are shown in black. The panels also show the censored intervals associated with the partisan and nonpartisan gatekeeping hypotheses. The blockout zone is shown in light gray, the gridlock interval in medium gray, and the intersection in dark gray.

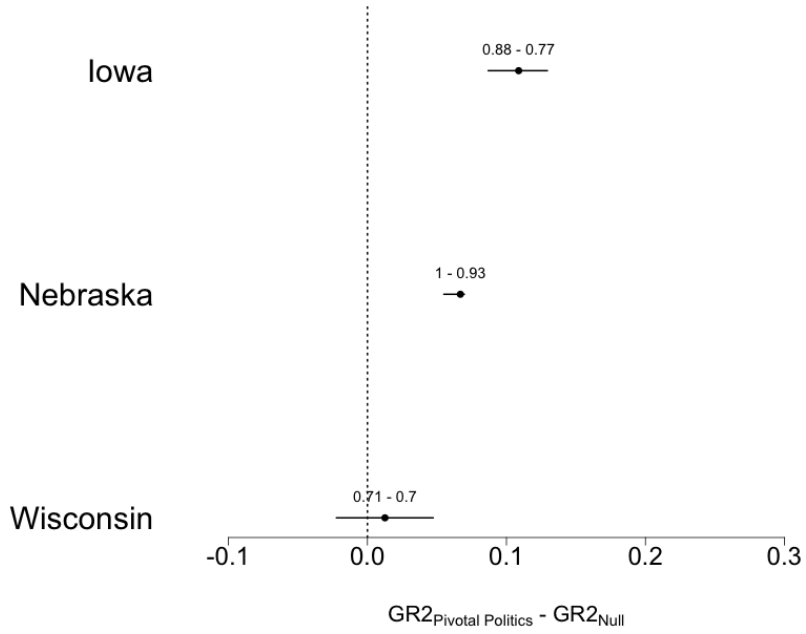
the Iowa Senate.

Tests of the Gridlock Hypothesis

In this section, I return to the explicit prediction of the pivotal politics theory, that no legislation will be enacted which has an associated status quo in the gridlock interval. In this interpretation, the gridlock interval is not a censored interval in that enactment, not floor consideration, of bills with status quos in the gridlock interval is a violation of the theory's prediction (see Peress 2013, 615). To test the prediction, I employ the *gridlock ratio* (GR2) which measures the proportion of bills with status quos in the gridlock interval that fail to be enacted. I compare this measure to a plausible null of random bill passage which is given by the overall proportion of estimated bills that fail to be enacted. Since enactment requires approval of the entire legislature and the governor, the gridlock ratio is measured at the state-level.

Figure 5 shows the results from tests of the gridlock hypothesis. For each state, the proportion of bills with estimates that are not enacted is subtracted from the gridlock ratio. We can reject the null of random bill passage in favor of the gridlock hypothesis in two out of three states. While the null is rejected in Iowa and Nebraska, the gridlock hypothesis does not perform better than chance in Wisconsin. Moreover, there is substantial variation in the extent to which there are violations of the gridlock hypothesis. In particular, whereas none of the status quo estimates from the Nebraska legislature violate the gridlock hypothesis, approximately 12% of estimates violate the assumption in Iowa. Of the estimates from the Wisconsin legislature, about 29% are not consistent with the gridlock hypothesis.

Figure 5: Tests of the Gridlock Hypothesis



Note: This figures presents results from tests of the pivotal politics gridlock hypothesis against the null hypothesis of random bill passage in Iowa, Nebraska, and Wisconsin. The first number over the estimates is the gridlock ratio. The second number is the proportion of bills with estimates that are not enacted.

Discussion

The analysis provides substantial evidence of nonrandom gatekeeping and, unsurprisingly, shows that many bills are introduced even though they are likely to blocked, suggesting either uncertainty or pure position-taking. In particular, the credible null of random gatekeeping can be rejected in favor of the pivotal politics-consistent hypothesis in all five chambers. Further, the party cartel prediction, when tested in isolation, finds support in both chambers of the Iowa and Wisconsin legislatures, but not in Nebraska. The null result in Nebraska is consistent with the party cartel theory in that it assumes that partisan legislative institutions, such as majority party committee leadership positions and legislative leaders with procedural rights, are a requirement for partisan gatekeeping. However, these institutions are not established in the nonpartisan Nebraska legislature. Therefore,

the results speak to the influence that institutions may have on legislative decision-making. More generally, the good performance of both hypotheses suggests that each theory provides a more convincing explanation of gatekeeping than an atheoretical account.

In a direct comparison of the party cartel and pivotal politics-consistent hypothesis, the former outperforms the latter in the lower chambers of the Iowan and Wisconsinite legislatures. This shows how parties use partisan legislative institutions to exercise negative agenda control, and influence the political process in lower chambers. Moreover, the tests cannot distinguish between the hypotheses in Nebraska and the state Senates of Iowa and Wisconsin. There are several possible reasons for the failure to distinguish between the hypotheses in these three chambers.

First, particularly in Wisconsin, chamber rules allow senators to circumvent gatekeeping by party leaders in a way that is similar to the U.S. Senate (e.g., Pearson 2008). Further, in Nebraska, the role of committees in screening out bills is limited since public hearings are generally required, and a majority of senators can withdraw bills from committees.[38] Moreover, it could be that – similar to senators in the U.S. Senate – the longer campaign cycle for legislators in upper chambers reduces electoral pressures (Jacobson and Carson 2016, Ch. 4), thereby motivating committee chairs and legislative leaders to screen out policy proposals that are unlikely to be enacted, rather than schedule votes on bills for the purpose of position-taking.[39] The results therefore speak to extant research which examines differences in party effects between the U.S. House and the U.S. Senate (e.g., Campbell, Cox, and McCubbins 2008; Monroe, Roberts, and Rhode 2008).

[38] In Nebraska, the inability to distinguish between the hypotheses may also partly be a result of the low proportion of status quo estimates for bills that receive floor consideration (13%).

[39] Based on the strong performance of the pivotal politics consistent gatekeeping hypothesis in the Iowa Senate, and Republican majorities in the Iowa House during most of time between 2005 and 2016, this account may be particularly relevant in the Iowa Senate. The focus on passing legislation is clear in the quote from former Senate Majority Leader Michael Gronstal (D) that during a time of split control of the Iowa Legislature lawmakers still "managed to pass property tax reform, health reform – there were many grand bargains made" (Kurtz 2017).

Since the pivotal politics model does not explicitly incorporate gatekeeping, one could argue that legislative gatekeeping is not the most appropriate ground for it to be tested on. Therefore, I conduct a supplemental analysis in which I directly test the theory's explicit prediction that there will be no policy change when a status quo for a proposed policy is located within the gridlock interval. I test the prediction against the null hypothesis of random bill (non)enactment and find support for it in Iowa and Nebraska, but not in Wisconsin. Apparent violations of the prediction in Wisconsin (and Iowa) allow for several interpretations, including the possibility of vote buying, party pressure (e.g., Volden and Bergman 2006; Krehbiel 1998, Ch. 8), unmeasured differences in bill quality (Hitt, Volden, and Wiseman 2017), or uncertainty about the state of the world (McCarty 2018).

The results presented in this paper also provide evidence on the subject of positive agenda control. Perhaps unsurprisingly, most bills that receive floor consideration are shown to have associated status quos that fall on the minority side of the ideological spectrum. This pattern, which is prominent in Iowa and Wisconsin, is consistent with a majority party leadership that promotes a record of legislative accomplishment to strengthen the party brand (Cox and McCubbins 2005). It is also consistent with an ideological argument for party labels in which a higher variance of the party member's positions is harmful to the party brand, and therefore harmful to the electoral chances of the party's candidates (Grynaviski 2010). Without a supermajority requirement to shut down a filibuster makes, it becomes a viable strategy to pass bills without bipartisan support (Pearson 2008, 119). As candidates for the state legislature in Nebraska are elected on a nonpartisan ballot, it is no surprise that the same pattern is not apparent for bills that receive floor consideration in the Nebraska legislature.

While the possibility of selection bias in the types of bills on which lobbying principals take

positions cannot be entirely ruled out, the positions from lobbying disclosures permit estimates of a large number of bills that are kept off the legislative agenda. The analysis therefore provides a new approach to address the critique that estimates generated by an endogenous agenda conforming to either the pivotal politics theory or the party cartel theory may be too imprecise to allow for distinctions between the theories (Clinton 2007).

I leave for future research and more thorough examination the question of whether partisan or non-partisan gatekeeping is more actively employed by Democrats or Republicans.[40] Since previous research has shown that legislators' utility functions in Congress can be estimated as Gaussian (Carroll *et al*. 13), a joint model of cosponsorship and voting with Gaussian utility would be a promising extension of the analysis in this paper. In addition, extant research suggests several additional gatekeeping and gridlock hypotheses to be tested (e.g., Cox and McCubbins 2005, Ch. 8; Chiou and Rothenberg 2003; Peress 2013, 614-616).

[40]Data from lobbying disclosures in additional years, as well as additional states that comprehensively disclosure lobbying principals' positions (Colorado, Montana, New Jersey, and Rhode Island) will provide greater purchase on this question.

References

Aldrich, John H. 1995. *Why parties?: The origin and transformation of political parties in America*. Chicago: University of Chicago Press.

American Political Science Association. 1950. *Toward a more responsible party system: A report.* New York: Rinehart.

Anzia, Sarah F., and Molly C. Jackman. 2013. "Legislative organization and the second face of power: Evidence from US State Legislatures." *Journal of Politics* 75(1): 210-224.

Badger, Emily, Quoctrung Bui, and Adam Pearce. 2018. "Republicans Dominate State Politics. But Democrats Made a Dent This Year." *The New York Times* November 10, 2018. Available at https://www.nytimes.com/interactive/2018/11/10/upshot/republicans-dominate-state-politics-but-democrats-made-a-dent.html (last accessed 06/23/19).

Bafumi, Joseph, Andrew Gelman, David K. Park, and Noah Kaplan. 2005. "Practical issues in implementing and understanding Bayesian ideal point estimation." *Political Analysis* 13(2): 171-187.

Brady, David W., and Craig Volden. 2006. *Revolving gridlock: Politics and policy from Jimmy Carter to George W. Bush*. 2nd ed. Boulder, CO: Westview Press.

Bryce, James. 1888. *The American commonwealth*. New York: Macmillan and Company.

Campbell, Andrea, Gary W. Cox, and Matthew D. McCubbins 2002. "Agenda power in the U.S. Senate, 1877 to 1986." In David W. Brady and Mathew D. McCubbins (Eds.): *Parties, procedure, and policy: Essays on the history of Congress*. Stanford University Press. Available at SSRN: https://ssrn.com/abstract=1009998.

Carpenter, Bob, Andrew Gelman, Matthew D. Hoffman, Daniel Lee, Ben Goodrich, Michael Betancourt, Marcus Brubaker, Jiqiang Guo, Peter Li, and Allen Riddell. 2017. "Stan: A probabilistic programming language." *Journal of Statistical Software* 76(1). Available at https://www.jstatsoft.org/article/view/v076i01 (last accessed 04/10/18).

Carroll, Royce, Jeffrey B. Lewis, James Lo, Keith T. Poole, and Howard Rosenthal. 2009. "Comparing NOMINATE and IDEAL: Points of difference and Monte Carlo tests." *Legislative Studies Quarterly* 34(4): 555-591.

Carroll, Royce, Jeffrey B. Lewis, James Lo, Keith T. Poole, and Howard Rosenthal. 2013. "The structure of utility in spatial models of voting." *American Journal of Political Science* 57(4): 1008-1028.

Chiou, Fang-Yi, and Lawrence S. Rothenberg. 2003. "When pivotal politics meets partisan politics." *American Journal of Political Science* 47(3): 503-522.

Clarke, Andrew J., Thomas R. Gray, and Kenneth Lowande. 2018. "Causal inference from pivotal politics theories." *Journal of Politics* 80(3): 1082-1087.

Clinton, Joshua D. 2007. “Lawmaking and roll calls.” *Journal of Politics* 69(2): 457-469.

Clinton, Joshua D. 2012. “Using roll call estimates to test models of politics.” *Annual Review of Political Science* 15: 79-99.

Clinton, Joshua D., Simon Jackman, and Douglas Rivers. 2004. “The statistical analysis of roll call data: A unified approach.” *American Political Science Review* 98(2): 355-370.

Cox, Gary W., Thad Kousser, and Matthew D. McCubbins. 2010. “Party power or preferences? Quasi-experimental evidence from state legislatures.” *Journal of Politics* 72(3): 799-811.

Cox, Gary W., and Matthew D. McCubbins. 2005. *Setting the agenda: Responsible party government in the U.S. House of Representatives*. Cambridge: Cambridge University Press.

Cox, Gary W., and Matthew D. McCubbins. 2007. *Legislative leviathan: Party government in the House*. Cambridge: Cambridge University Press.

Crombez, Christophe, Tim Groseclose, and Keith Krehbiel. 2005. “Gatekeeping”. *Center for Institutions and Governance Working Paper* 9. UC Berkeley: Institute of Governmental Studies. Available at http://escholarship.org/uc/item/7b30r8xv.

Crombez, Christophe, Tim Groseclose, and Keith Krehbiel. 2006. “Gatekeeping.” *Journal of Politics* 68(2): 322-334.

Gailmard, Sean, and Jeffery A. Jenkins. 2007. “Negative agenda control in the Senate and House: Fingerprints of majority party power.” *Journal of Politics* 69(3): 689-700.

Grynaviski, Jeffrey D. 2010. *Partisan bonds: Political reputations and legislative accountability*. Political Economy of Institutions and Decisions Cambridge: Cambridge University Press.

Hirsch, Alexander V. 2011. “Theory driven bias in ideal point estimates - A Monte Carlo study.” *Political Analysis* 19(1): 87-102.

Hirsch, Alexander V., and Kenneth W. Shotts. 2015. “Competitive policy entrepreneurship.” *American Economic Review* 105(4): 1646-1664.

Hitt, Matthew P., Craig Volden, and Alan E. Wiseman. 2017. “Spatial models of legislative effectiveness.” *American Journal of Political Science* 61(3): 575-590.

Jackman, Molly C. 2014. “Parties, median legislators, and agenda setting: How legislative institutions matter.” *Journal of Politics* 76(1): 259-272.

Jackman, Simon. 2015. *pscl: Classes and methods for R developed in the Political Science Computational Laboratory, Stanford University*. Department of Political Science, Stanford University. Stanford, California. R package version 1.4.9. Available at http://pscl.stanford.edu/.

Jacobson, Gary C, and Jamie L. Carson. 2016. *The politics of congressional elections*. Lanham: Rowman & Littlefield.

Jenkins, Jeffery A., and Nathan W. Monroe. 2016. "On measuring legislative agenda-setting power." *American Journal of Political Science* 60(1): 158-174.

Krehbiel, Keith. 1996. "Institutional and partisan sources of gridlock." *Journal of Theoretical Politics* 8(1): 7-40.

Krehbiel, Keith. 1998. *Pivotal politics: A theory of U.S. lawmaking*. Chicago: University of Chicago Press.

Krehbiel, Keith. 2007. "Partisan roll rates in a nonpartisan legislature." *Journal of Law, Economics, and Organization* 23(1): 1-23.

Krehbiel, Keith, Adam Meirowitz, and Alan E. Wiseman. 2015. "A theory of competitive partisan lawmaking." *Political Science Research and Methods* 3(3): 423-448.

Krehbiel, Keith, Adam Meirowitz, and Jonathan Woon. 2005. "Testing theories of lawmaking." In David Austin-Smith and John Duggan (Eds.): *Social choice and strategic decisions: Essays in honor of Jeffrey S. Banks*. Heidelberg, Germany: Springer-Verlag.

Krehbiel, Keith, and Zachary Peskowitz. 2015. "Legislative organization and ideal-points bias." *Journal of Theoretical Politics* 27(4): 673-703.

Kurtz, Karl (2017). "Why states can get things done when Congress can't." *NCSL State Legislatures Magazine* July/August 2017. Available at http://www.ncsl.org/bookstore/state-legislatures-magazine/legislatures-are-polarized-not-paralyzed.aspx.

Kypriotis, Chris. 2013. "Theories of legislative politics and committee action in the postwar U.S. House of Representatives." *Journal of Theoretical Politics* 25(2): 234-259.

Lax, Jeffrey R., and Justin H. Phillips. 2012. "The democratic deficit in the states." *American Journal of Political Science* 56(1): 148-166.

McCarty, Nolan. 2018. "Pivotal politics, partisan polarization, and policy predictability." *Journal of Politics* 80(3): 1076-1081.

Londregan, John B. 2000. "Estimating legislators' preferred points." *Political Analysis* 8(1): 35-56.

Miller, Warren E., and Donald E. Stokes. 1963. "Constituency influence in Congress." *American Political Science Review* 57(1): 45-56.

Monroe, Nathan W., Jason M. Roberts, and David W. Rohde (Eds.) 2009. *Why not parties?: Party effects in the United States Senate*. Chicago: University of Chicago Press.

Pearson, Kathryn. 2008. "Party loyalty and discipline in the individualistic senate." In Nathan Monroe, David Rohde, and Jason Roberts (Eds.): *Why not parties? Party effects in the United States Senate*. Chicago: University of Chicago Press.

Peress, Michael. 2013. "Estimating proposal and status quo locations using voting and cosponsorship data." *Journal of Politics* 75(3): 613-631.

Poole, Keith, Jeffrey Lewis, James Lo, and Royce Carroll. 2011 "Scaling roll call votes with wnominate in R." *Journal of Statistical Software* 42(14): 1-21.

Poole, Keith T., and Howard Rosenthal. 2007. *Ideology and Congress*. New Brunswick: Transaction Publishers.

Richman, Jesse. 2011. "Parties, pivots, and policy: The status quo test." *American Political Science Review* 105(1): 151-165.

Schaffner, Brian F. 2007 "Political parties and the representativeness of legislative committees." *Legislative Studies Quarterly* 32(3): 475-497.

Schattschneider, Elmer E. 1942. *Party Government*. New York: Rinehart and Company.

Schickler, Eric, and Kathryn Pearson. 2009. "Agenda control, majority party power, and the House Committee on Rules, 1937-52." *Legislative Studies Quarterly* 34(4): 455-491.

Snyder, James M. 1992a. "Committee power, structure-induced equilibria, and roll call votes." *American Journal of Political Science* 36(1): 1-30.

Snyder, James M. 1992b. "Artificial extremism in interest group ratings." *Legislative Studies Quarterly* 17(3): 319-345.

Stan Development Team. 2017. *RStan: the R interface to Stan*. R package version 2.16.2. Available at http://mc-stan.org.

Stiglitz, Edward H., and Barry R. Weingast. 2010. "Agenda control in Congress: Evidence from cutpoint estimates and ideal point uncertainty." *Legislative Studies Quarterly* 35(2): 157-185.

Thieme, Sebastian. Forthcoming. "Moderation or Strategy? Political Giving by Corporations and Trade Groups." *Journal of Politics*. Available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3432733.

Thulin, Måns. 2014. "Decision-theoretic justifications for Bayesian hypothesis testing using credible sets." *Journal of Statistical Planning and Inference* 146: 133-138.

Treier, Shawn. 2010. "Where does the president stand? Measuring presidential ideology." *Political Analysis* 18(1): 124-136.

Volden, Craig, and Elizabeth Bergman. 2006. "How strong should oOur party be? Party member preferences over party cohesion." *Legislative Studies Quarterly* 31 (1): 71-104.

Woon, Jonathan. 2008. "Bill sponsorship in Congress: The moderating effect of agenda positions on legislative proposals." *Journal of Politics* 70(1): 201-216.

Woon, Jonathan, and Ian P. Cook. 2015. "Competing gridlock models and status quo policies." *Political Analysis* 23(3): 385-399.

A Direct Test of Legislative Gatekeeping

(Online Appendix)

Table of Contents

Appendix A: Details About Lobbyist Disclosures

All 50 states currently have reporting requirements for state-level lobbyists. However, the extent of required disclosure varies markedly across states.[41] Lobbying rules in Iowa, Nebraska, and Wisconsin are rare in that they require lobbyists to report the bills and resolutions on which they lobby legislators, as well as the principals on whose behalf they lobby on each bill or resolution.[42] Further, lobbyists in Iowa and Nebraska are required to report the positions they communicated towards legislators on behalf of their principals. Although lobbyists in Wisconsin are not required to disclose positions, they do so in a large majority of cases. Between 2003-2016, in 83.3% of cases where lobbyists registered an interest in a bill or resolution, they also reported a position.

The format in which positions are reported is different in each state. In Iowa, lobbyists must choose between *For*, *Against*, and *Undecided* within one day of lobbying a legislator.[43] Similarly, they must report any change in the communicated position within one working day.[44] Lobbyists report lobbied positions through an online system, and the *lobbyist declarations* are made publicly accessible on the website of the Iowa State Legislature. A declaration also includes the name of the bill or resolution, the lobbyist's name, the principal's name, and the date of the declaration.[45] Each bill in the online *BillBook* has a link to the relevant lobbyist declarations.[46] Unlike Nebraska and Wisconsin, Iowa requires officials representing state offices and agencies to register their positions.

Wisconsin has a similar reporting system to Iowa. Lobbyists must register their principals' in-

[41] A state-by-state overview is available from the National Conference of State Legislatures at http://www.ncsl.org/research/ethics/50-state-chart-lobbyist-registration-requirements.aspx.

[42] See Iowa Code §68B.36 and lobbyist rules passed in the Iowa state legislature, §49-1488 of Nebraska Revised Statutes, and Chapter 13 of the Wisconsin Statutes.

[43] *Undecided* is also used to indicate that a lobbyist is monitoring a bill of (potential) interest to his or her principal.

[44] Since 2015, lobbyists may also choose *Withdraw* as a position to indicate that the principal is no longer interested in the legislation. However, previous declarations will remain visible.

[45] Information on the time of the declaration is available from 2009.

[46] https://www.legis.iowa.gov/legislation/BillBook

terest within 15 days of first communicating with a legislator. Furthermore, lobbyists who choose to report their principals' position can choose between *For*, *Against*, *Other*, and *Undisclosed*. In addition, lobbyists can upload documents and links to documents in support of their position, as well as a comment with a maximum of 250 characters. These statements are immediately made publicly accessible through the website *Eye on Wisconsin*, currently maintained by the Wisconsin Ethics Commission.[47] As in Iowa, lobbyists in Wisconsin may amend their principals' reported position at any point in the legislative process, with previously reported positions remaining publicly accessible. The online database shows the date a position was first reported or amended. Differently from Iowa and Nebraska, it does not show which lobbyist reported the position.

In Nebraska, lobbyists must register the positions they communicated on behalf of their principals on a *Statement of Activity* (Nebraska Accountability and Disclosure Commission Form D) within 45 days of the end of a legislative session (see §49-1488). Lobbyists are required to report communicated positions on bills. Whereas in Iowa and Wisconsin the reported positions refer to the version of the bill that is current at the time, the reported positions in Nebraska generally refer to the last version of the bill. However, some lobbyists also report their principals' positions at different stages of the legislative process (e.g., *Support as Introduced, Oppose as Amended*).[48] Electronic versions of filed forms are available on the website of the Nebraska state legislature.[49]

I collected and assembled all lobbyist declarations from Iowa between 2005 and 2016, all statements of activity from Nebraska between 2003 and 2016, and all principal lobbying efforts in Wisconsin between 2003 and 2016. I do not include declarations from Iowa from 2003 through

[47] The website is available at https://lobbying.wi.gov/Home/Welcome. The Government Accountability Board was replaced by the Elections Commission and the Ethics Commission on June 29, 2016, pursuant to Wisconsin Act 118.

[48] Since 2015, lobbyists in Nebraska must submit statements of lobbying activity electronically, which limits their options to *Support*, *Oppose*, and *Neutral* (LB 782, 2012). Prior to 2013, many statements were submitted manually, allowing for more variation in how positions were described.

[49] See http://nebraskalegislature.gov/lobbyist/view.php?v=principal.

2004, as these do not include information on the lobbyists' principals and the date when the declaration was made. I also do not include statements from Nebraska before the 2003 session to maintain a relative balance in the time period across states.[50]

Appendix B.1: Combining Declarations with Roll Call Votes

Since introduced bills are frequently amended in the legislative process, linking lobbyist declarations to roll call votes on bill passage requires making assumptions about which version of a bill a declared position refers to. I employ bill histories in Iowa, Nebraska, and Wisconsin to determine the dates of successful amendments so as to identify which bill version was current at a particular date.[51] I assume that any successful amendment constitutes a change in the bill version.[52]

Next, I assume that declarations apply to the then-current bill versions. Therefore, I do not assume that declarations refer to previous bill versions. In Iowa and Wisconsin, where position statements made throughout the legislative process are reported by date, I assume that a declaration made at a time when a previous bill version was current carries over to subsequent versions until a new declaration replaces it.[53] Any bill version that was current for a day or less is disregarded, under the assumption that potential changes in the principal's position cannot be picked

[50]The Nebraska state legislature makes all statements of activity since 2001 available on its website. The current *Eye on Wisconsin* website shows lobbying efforts from 2003-2016. Prior lobbying efforts are available in paper format from the State of Wisconsin Historical Society (see http://www.gab.wi.gov/lobbying).

[51]Bill histories identify the dates of legislative actions, e.g., introductions, committee referrals, committee reports, filed or adopted amendments, or floor votes. In Iowa, bill histories are available at https://www.legis.iowa.gov/legislation/billTracking/billHistory. In Iowa, I also merge bill histories and declarations for study bills and subsequently introduced identical bills. Further, I merge bill histories and the declaration records for identical companion bills within the same chamber. In Nebraska, bill chronologies are available by session (e.g., http://www.nebraskalegislature.gov/session/view_archives.php?leg=98 for the 98th Legislature, 2003-2004). In Wisconsin, bill histories are listed on bill websites (e.g., http://docs.legis.wisconsin.gov/2015/proposals/reg/asm/bill/ab1 for AB 1 in the 2015-2016 legislature).

[52]Another way to proceed would be to distinguish between contentious and non-contentious amendments.

[53]In Iowa, since I aggregate declarations of multiple lobbyists for the same principal, and since lobbyists tend to make initial *Undecided* declarations before declaring support or opposition, I exclude all such declarations by lobbyist 1, for principal A on bill X, unless lobbyist 1 previously declared support for or opposition to bill X for principal A.

up in such a short interval. However, declarations from the day of a successful amendment are not disregarded but matched to subsequent bill versions if these last longer than one day.[54]

In Nebraska, position statements are generally made after the end of a session. Therefore, I assume that positions such as *Support* or *Oppose*, without reference to amendments or specific bill versions, apply only to then-current bill versions. This reduces the number of positions on initial bill versions for bills that were subsequently amended. I use the bill histories to determine the bill version current at the end of the session. However, whenever a more detailed position was provided, such as *Oppose as Introduced, Support as Passed*, I manually code these accordingly, i.e., with a *nay* vote on the first bill version and a *yea* vote on the last bill version. I also manually code positions on amendments or procedural motions, when they can be matched to a roll call vote. Any discrepant position on behalf of a principal that cannot be reconciled is coded as missing.[55]

In the ideal point estimation, I only use lobbyist declarations that indicate a position for or against a motion. Where any other positions are linked to a bill version or motion, I code the position as missing. This is done to simplify the analysis, as other response categories are harder to interpret. In particular, the position *Undecided* in Iowa may indicate that a lobbyist is monitoring a bill, rather than a genuinely undecided position. In Wisconsin, the *Other* position may indicate support for parts of the bill and an opposition to others, support for or opposition to an amendment, a neutral position, or a general interest. Similarly, the *Neutral* position in Nebraska, may indicate monitoring of bills, support and opposition of different parts, as well as well as a middle posi-

[54]These assumptions imply active lobbyists that follow the progress of bills and change their declarations if their principals' positions change. I assume that lobbyists in Iowa and Wisconsin use the online reporting system to communicate changes in their principals' positions to legislators as quickly as possible. In Iowa, this assumption is also motivated by Rule 2.2 of the Joint Rules Governing Lobbyists (HJR 7 2015-2016), requiring lobbyists to declare changes in their principal's position within a business day. Results from robustness tests in which I make different assumptions when merging declarations with bill versions, e.g., that declarations refer to the first bill version or only the then-current bill version are available upon request.

[55]This affects less than 50 positions.

tion.[56] Without additional information, naive coding of such "third" positions as a middle category between *Oppose* and *Support* is likely to result in biased estimates (e.g., Lo 2013).[57] A similar argument applies to abstentions (e.g., Rosas, Shomer, and Haptonstahl 2015).

In the final step, I construct the legislator-principal-vote matrix from the matched position statements by combining the declarations on bill versions with legislator roll calls whenever a bill version is associated with a final passage roll call vote. Whenever a bill version is not associated with a roll call vote, e.g., because it died in committee, I add the positions associated with the bill version to the matrix as a separate column. Therefore, declarations on bills without floor consideration are coded as "votes" on the initial version of the bill.[58]

Appendix B.2: Committee Registrations in Wisconsin

In Wisconsin, for bills on which a lobbyist chose not to disclose the position of a lobbying communication, but registered a position at a committee hearing, I determine the principal's position by combining the committee registrations with the online lobbying disclosures. Moreover, I combine legislators' positions registered at committee hearings with the corresponding bill version.[59]

Since hearings in the non-originating chamber are often on already amended bills, I use bill histories to link positions to the correct bill versions. I do not assume that positions from committee registrations carry over to subsequent versions, since committee registrations, unlike positions on

[56]The use of the *Neutral* position in Nebraska increased from about 900 in 2012 to about 12,500 in 2016, while the combined number of supporting and opposing positions stayed roughly the same across the years.

[57]In a robustness check where the response category *Undecided* is naively coded as a middle category between *For* and *Against*, and Iowa legislators are scaled together with principals' positions on bills in Iowa, the results lack face-validity. In particular, all principals' ideal point estimates are located in a very narrow band between all Democrats and all Republicans. Similarly, results from Nebraska when *Neutral* is treated as a middle category lack face-validity in that almost all principals, including unions, have more conservative estimates than the most conservative legislator.

[58]I consider a bill as unamended until the amendment has been passed by the chamber.

[59]Committee votes in the Iowa Senate, and the Nebraska and Wisconsin legislatures are additional potential sources of position data. The Iowa House does not publish the votes of individual committee members.

"Eye on Lobbying", cannot be amended without another hearing. Since appearing for information does not indicate a position, I code such registrations as missing votes. Using bill versions, I then augment the vote matrix by filling in missing votes with positions from committee registrations.

Appendix B.3: Identity of Principals Across Time

Merging principals' position-records across legislative sessions requires assumptions about the identity of a principal.[60] I use the principals' registered names, addresses, and websites to identify matches across time.[61] I do not merge subsidiary and parent (or holding) companies, unless registrations indicate a representation by the parent company. If one company with a registered lobbyist acquired another company with a registered lobbyist and the registration record did not reflect this change, I change the name to reflect either its new subsidiary name, or, if the acquired company was likely represented by a new parent company, to the name of the new parent company.

For municipalities, school districts, public power districts, and state-wide offices, I use the identity of the mayor, superintendent, president, or office-holder to create separate voting records. For county governments, I use changes in partisan control, and for state boards with leaders appointed by the governor, I use the identity of the governor to create separate voting records. In Iowa, I merge state agency positions with the position record of the then-current governor.

Appendix C.1: The Joint Model of Cosponsorship and Voting

To estimate the status quo positions of proposed bills, I combine data from roll call votes, lobbying declarations, bill cosponsorships, and legislator covariates to estimate a joint statistical model of

[60] This issue does not usually arise in item-response analysis, because the subjects tend to be individuals.

[61] In Nebraska, I also use an official directory of name changes to identify name changes among principals.

voting and bill cosponsorship, which closely follows the model developed by Peress (2013). The first part of the model consist of a standard item-response model (e.g., Bafumi *et al.* 2005; Clinton, Jackman, and Rivers 2004), which relies on a vote matrix to estimate the cutpoint and discrimination parameters of votes, as well as ideal points of elected officials and lobbying principals.[62] The second part of the model uses bill cosponsorships, bill-fixed effects, and legislator covariates to estimate the positions of bill proposals on the same ideological dimension as that recovered from the voting model.[63] The log-likelihood function for the data is given by:

$$\begin{aligned} l(\alpha, \beta, p, q, \delta, \tau, \theta) = & \sum_{n=1}^{N} \Big\{ \sum_{t \in T_v} y_{n,t} \log F(\beta_t(\theta_n - \tau_t)) + (1 - y_{n,t}) \log(1 - F(\beta_t(\theta_n - \tau_t))) \Big\} \\ & + \sum_{n=1}^{N} \Big\{ \sum_{t \in T_c} y_{n,t} \log F(\frac{-(p_t - \theta_n)^2 - \alpha' x_n - q_t}{\delta_t}) \\ & + (1 - y_{n,t}) \log(1 - F(\frac{-(p_t - \theta_n)^2 - \alpha' x_n - q_t}{\delta_t})) \Big\}. \end{aligned}$$

Here, N is the number of legislators, T_v the set of voting decision indices, and T_c the set of cosponsorship decision indices. The first part of the model represents the vote choice $y_{n,t}$ of actor n on vote t. This part of the model identifies the ideal point of actor n (θ_n) and the cutpoint of vote t (τ_t). Further, $\beta_t = 2(p_t - s_t)/\sigma_t$, where p_t is the proposal location ("yea"), s_t is the status quo ("nay"), and σ_t is the standard deviation of the differences between the errors of voting for p_t or s_t.

A legislator with an ideal point at a vote's cutpoint (also known as midpoint) is indifferent between voting for p_t and s_t.[64] Given assumptions about σ_t, the first part of the model also identifies the location of the proposal and the status quo (Carrol *et al.* 2009). For example, if $\sigma_t = 1$, since

[62] For bills without floor consideration, estimates are based on lobbying declarations. For bills with floor consideration, estimates are based on roll call votes and lobbying declarations.

[63] Following Peress (2013, 619), I restrict attention to bills with at least 3 cosponsors.

[64] The cutpoint τ_t can be rewritten as $\frac{p_t^2 - s_t^2}{\sigma_t} / \frac{2(p_t - s_t)}{\sigma_t}$ or $\frac{\alpha_t}{\beta_t}$.

$s_t = \frac{p_t+s_t}{2} - \frac{p_t-s_t}{2}$, it follows that $s_t = \tau_t - \beta_t/4$.

The second part of the equation models the choice of legislator n to cosponsor bill p_t based on quadratic loss as distance to her ideal point increases, a bill-specific fixed effect (q_t), and a vector of legislator-specific covariates, x_n. The underlying assumption is that unlike bill *sponsorship*, bill *cosponsorship* is nonstrategic, and used by legislators to signal their preferences (e.g., Woon 2008). Moreover, cosponsorship is assumed to be governed by the same ideal points as voting (Peress 2013). Legislator n will sponsor bill t if the utility from cosponsoring is greater than a random threshold, for which the mean varies by legislator and bill.[65] Further, x_n accounts for party membership and whether the legislator was a majority or minority party leader of the committee to which the bill was referred.[66] Finally, the bill-fixed effect q_t allows the salience of bills to vary and accounts for large differences in the likelihood of cosponsorships across bills (Peress 2013, 618).

Based on the assumption of symmetric preferences, the status quo can be calculated as the reflection of the proposal location on the estimated cutpoint, i.e. $\hat{s}_t = 2\hat{\tau}_t - \hat{p}_t$ (Peress 2013). This approach provides an alternative to estimating status quos only via voting data and does not rely on assumptions about σ_t for identification. However, since many bills lack cosponsors it limits the number of bills for which status quos can be estimated with sufficient accuracy (Peress 2013, 619).

To estimate status quos for a larger set of proposed bills, I use adjusted vote-based status quo estimates when there are not enough cosponsorships for a sufficiently accurate estimate of p_t. In particular, instead of relying on the strong assumption that the error standard deviation σ is homogenous, I employ parameter estimates from the joint model to estimate $\hat{\sigma}_g$, for a group g of bills based on the state, chamber, session, and majority vs. non-majority party sponsor.[67] I then adjust

[65] That is, if $-(p_t - \theta_n)^2 > \alpha_n' x + q_t + \epsilon^c_{n,t}$, where the error term $\epsilon^c_{n,t}$ has cdf $F(\epsilon/\delta_t)$, and $F = logit^{-1}$.

[66] The Nebraskan legislature does not distinguish committee leaders by majority or minority party.

[67] Since there are no $\hat{\sigma}_g$ for bills sponsored by minority party members in Iowa, I adjust all available estimates based on state, chamber, and session.

the vote-based status quos using estimates of σ_g.

In particular, since $\beta_t = 2(p_t - s_t)/\sigma_t$, $\hat{\sigma}_t$ can be estimated by $2(\hat{p}_t - \hat{s}_t)/\hat{\beta}_t$ from the joint model. I then estimate $\hat{\sigma}_g$, as the median of all σ_t within a group of bills, i.e., $\hat{\sigma}_g = \text{Median}(\hat{\sigma}_t | t \in g)$.[68] Since the original estimation of the model assumed $\sigma = 1$, s will be biased if this assumption is not met.[69] Therefore, I calculate the adjusted status quos as $\hat{as}_t = \hat{\tau}_t - \hat{\beta}_t/4 * \hat{\sigma}_{g(t)}$, where $g(t)$ is the group of vote t.

Appendix C.2: Estimation of the Joint Model in Stan

The analysis is conducted using three within-state estimations. This is done to simplify the estimation and increase the precision of cutpoint estimates, especially in Nebraska, where legislators are less polarized than in Iowa or Wisconsin (Thieme Forthcoming). To balance the trade-off between precision of the parameter estimates and estimating a substantial number of status quos, I reduce the vote matrix so that all "votes" have a minimum number of 9 actors voting on it, all actors have at least 9 votes, and the minority vote is greater or equal to the maximum of 2 votes or 2.5% of the votes. Votes that do not meet these requirements are excluded. In cases where all legislators vote or abstention is not strategic, unanimous roll calls are unlikely to capture a left-right dimension. Instead, they may capture deference to the chamber majority or non-partisan inter-chamber conflict (Poole and Rosenthal 2007, 230). To prevent bias that can arise from legislators and lobbying principals having different item parameters (e.g., Jessee 2016), I exclude votes which combine unanimous roll calls with opposing positions from lobbying principals from the vote matrix.

I implement the joint model using the probabilistic programming language Stan (Carpenter *et*

[68] Since I estimate $\hat{\sigma}_g$ from the draws of posterior distribution and also adjust status quos for each draw, I recode $\hat{\sigma}_g$ to 1 in the very small number of iterations it is negative.

[69] Remember that $s = \tau - \beta/4$ or $s = \tau - \frac{2(p-s)}{4\sigma}$ is only the same as $s = \frac{p+s}{2} - \frac{p-s}{2}$ if $\sigma = 1$.

al. 2017) via the R package *rstan* (Stan Development Team 2017) which is designed for Bayesian inference. The combination of Hamiltonian Monte Carlo and No-U-Turn-Sampler (Hoffman and Gelman 2014) increase the efficiency of sampling by avoiding a random-walk behavior and addressing the issue of correlated parameters. Due to this high level of efficiency, I rely on three chains with burn-in periods of 750 iterations, drawing the subsequent 750 samples from the posterior distribution in Iowa and Wisconsin. In Nebraska, where convergence is somewhat slower, I rely on 1500 iterations for the burn-in period and sampling.

The variables have the following independent priors (mean, standard deviation): $p \sim N(0,1)$, $\theta \sim N(0,1)$, $\tau \sim N(0,1)$, $q \sim N(0,5)$, $\beta \sim N(0,5)$. The parameters for legislator covariates the following priors: $party \sim N(0,1), com\_maj\_leader \sim normal(0,2), com\_min\_leader \sim N(0,2)$. Moreover, $\delta \sim \gamma(v,v)$, where $\nu \sim N(20,3)$. Further, I constrain the parameters to the following intervals: $\tau \in (-2.5, 2.5), \beta \in (-35, 35), p \in (-2.5, 2.5), \delta \in (0, 15), v \in (0, 15)$.

Starting values for θ, τ, β, are obtained using the function *ideal* in the R package *pscl* (Jackman 2015). Starting values for p are set as the median of cosponsor ideal points. Starting values for q are obtained by z-scoring the number of cosponsors on a bill. Starting values for party are obtained similarly. Starting values for committee leadership positions are set at -1 (for a positive effect). Starting values for δ are drawn from a gamma distribution similar to the prior distribution for γ.[70]

Appendix C.3: Convergence Diagnostics

Visual inspection of the traceplots and the potential scale reduction factor (PSRF/$\hat{R}$) (Gelman and Rubin 1992) show a high level of convergence in the Markov Chains. The $\hat{R}$ are calculated with all three MCMC chains. Table 1 shows, for each of the three within-state estimations, the number

[70]For each parameter, a random error term was added to the starting values of chains 2 and 3.

and percentage of parameters for which $\hat{R} > 1.1$.

Table 1: Percentage and Number of Parameters with Potential Scale Reduction Factor $\hat{R} > 1.1$

Estimation	$\%\hat{\theta}$	$\#\hat{\theta}$	$\%\hat{\beta}$	$\#\hat{\beta}$	$\%\hat{\tau}$	$\#\hat{\tau}$	$\%\hat{p}$	$\#\hat{p}$
Iowa	0.1	1	0.3	14	0.5	23	0.2	4
Nebraska	0.4	2	1.3	31	1.9	45	0	0
Wisconsin	0.1	1	0.2	14	0.3	17	< 0.1	6

Note: This table shows, for each within-state estimation, the percentage and number of parameters for which $\hat{R} > 1.1$.

Appendix D: Comparisons Between Different SQ Estimates

Absent auxiliary information from bill co-sponsorships, status quo and proposal locations in the joint model of cosponsorship and voting are only identified by the choice of the standard deviation of the difference in errors of voting for the status quo and the proposal, σ_t.[71] For the initial estimations, I assume that $\sigma_t = 1, \forall \sigma_t$. Due to the need to rely on such strong assumptions, Carrol *et al*. (2009) advise against using bill and status quo estimates from voting models without auxiliary evidence. In the following, I compare status quos estimated solely from vote choices to status quos estimated jointly via vote choices and cosponsorships.

In addition, I compare both sets of estimates to status quos that rely on bill cutpoints and assumed bill positions. In particular, I employ the shortcut of assuming that the proposal location is given by the ideal point of the bill sponsor (Peress 2013, 622-623).[72] Below, I compare status quo estimates which rely on this assumption to status quos estimated via votes and cosponsorships.[73]

[71] NOMINATE models identify the proposal and status quo location from the data via the choice of a scaling parameter and the non-linearity of the choice function (Carroll *et al*. 2009, 567).

[72] The Wisconsin legislature distinguishes between coauthors (co-introducers from the same chamber) and cosponsors (co-introducers from the other chamber). In my analysis, I treat coauthors as cosponsors. For bills introduced by state agencies in Iowa, I code the bill position as that of the governor's ideal point.

[73] As in the case of cosponsorship-based estimates, this approach relies on the equation $s_t = 2\tau_t - p_t$.

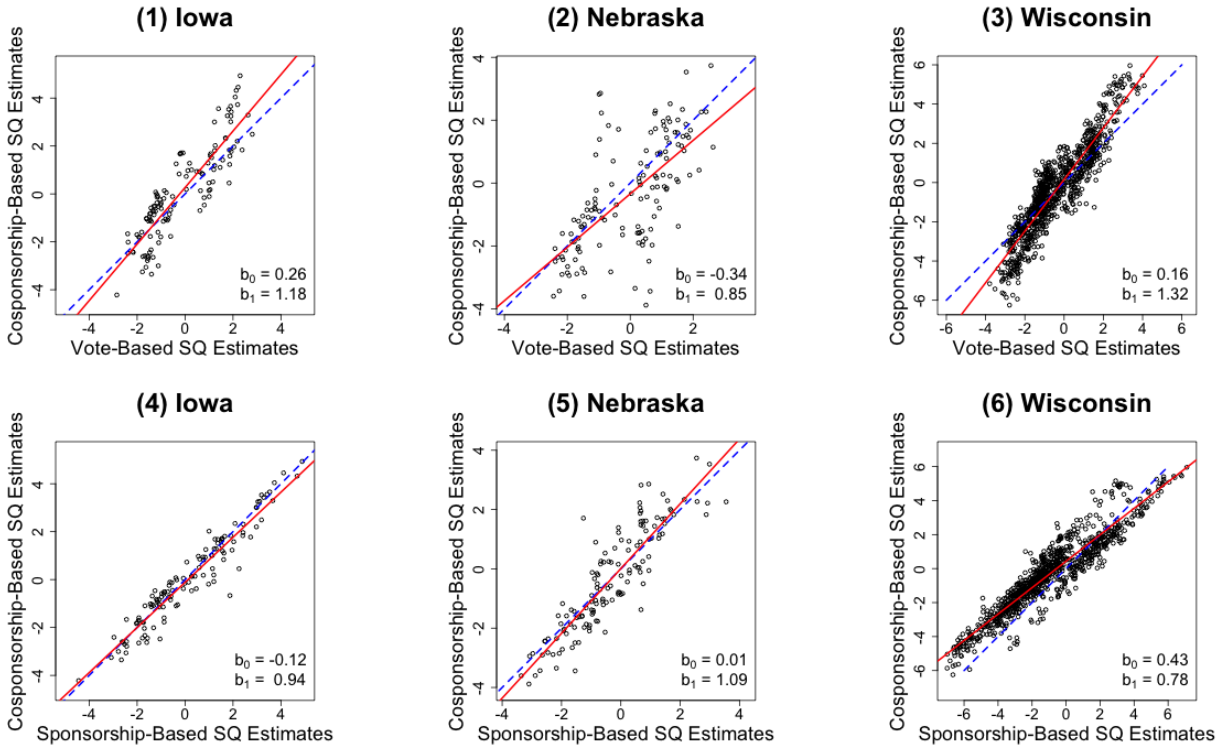
Panels 1-3 in Figure 1 shows scatterplots between status quos estimated via cosponsorship and status quos estimated via voting without adjustment. The correlations are 0.88 (Iowa), 0.66 (Nebraska), and 0.91 (Wisconsin). In comparison with Figure 2 in the main analysis, this indicates that if the bill-group-specific $\hat{\sigma_g}$ are good estimates of σ_g, the adjustment of the vote-based estimates via $\hat{\sigma}_g$ will improve the approximation of cosponsorship-based estimates with vote-based estimates.

Panels 4-6 in Figure 1 compare status quos estimated via cosponsorship with status quos estimated via assumed bill positions. The correlations for each of the states are 0.96 (Iowa), 0.89 (Nebraska), and 0.94 (Wisconsin). Interestingly, these correlations are stronger than those between cosponsorship-based estimates and either adjusted or the unadjusted vote-based estimates. Moreover, the comparison in Iowa and Nebraska (but not in Wisconsin) closely tracks the 1:1 line. This suggests that at least for the estimates in the sample, bill sponsorship in Iowa and Nebraska are not as strategic as in Wisconsin. Results using cosponsorship-based estimates, unadjusted vote-based estimates, and sponsorship-based estimates are presented in Appendices F.1 and F.2.

Appendix E.1: Rules on Agenda Control in Iowa and Wisconsin

In Iowa and Wisconsin, chamber rules put majority party legislators in positions to block legislation, in a way that is largely consistent with the party cartel theory (Cox and McCubbins 2005, Chapter 3). First, agents of the majority party select committee chairs (Iowa House Rule 46, Iowa Senate Rule 34, Wisconsin Assembly Rule 9(2)(b), and Wisconsin Senate Rule 20(2)(a)). Second, procedural rules put committee chairs in these chambers in a position to delay or kill legislation, since they can choose not to hold a committee hearing, and are not required to report all bills to the floor (Anzia and Jackman 2013). Third, the same is true to some extent for rules on calendar

Figure 1: Adjusted Status Quo Estimates Versus Cosponsorship-Based Status Quo Estimates



Note: Panels 1-3 compare vote-based status quo estimates assuming a homogenous σ (horizontal axis) with status quos that are estimated using bill cosponsorship decisions and votes (vertical axis). In the second row, the estimates on the horizontal axis are based on assumed bill positions. The dashed blue lines represent 45-degree lines, and the solid red lines represent the respective regression lines. The regression coefficients are shown in the bottom right corner of each panel. In each row, the number of comparisons is 124 in Iowa, 135 in Nebraska, and 922 in Wisconsin.

scheduling by agents of the majority party.

To be sure, none of chambers give absolute veto rights to committees, and each chamber has a discharge procedure to withdraw bills from committee. In the Iowa House (Senate), bills that have been in committee for eighteen (fifteen) legislative days may be withdrawn by a majority of elected members (Iowa House Rule 60 and Iowa Senate Rule 42). In Iowa, the House Speaker and the majority leader in the Senate can prevent a floor vote by re-referral of the bill to committee (House Rule 43 and Senate Rule 33(5)). The Iowa Senate majority leader can also chose whether to schedule a vote (Senate Rule 6(4) and Senate Rule 6(7)). Arguably, this institutional setup may

more closely resembles a veto game rather than a gatekeeping game (Crombez, Groseclose, and Krebiehl 2006).

In Wisconsin, discharge motions and petitions are limited by the amount of time a bill has been in committee and whether a public hearing has been scheduled (Assembly Rule 15 and Senate Rule 41). With respect to re-referral of bills and scheduling, Assembly Rule 42(1)(c) allows the Speaker to re-refer a bill to committee even if it has been withdrawn by a majority. On the other hand, Senate Rule 41(1)(c) not only gives a majority the right to withdraw a bill from committee, but also to place it on the next succeeding calendar. However, after an initial failure of a discharge motion, any further attempt requires a two-thirds majority (Assembly Rule 15(3) and Senate Rule 41(1)(b)).

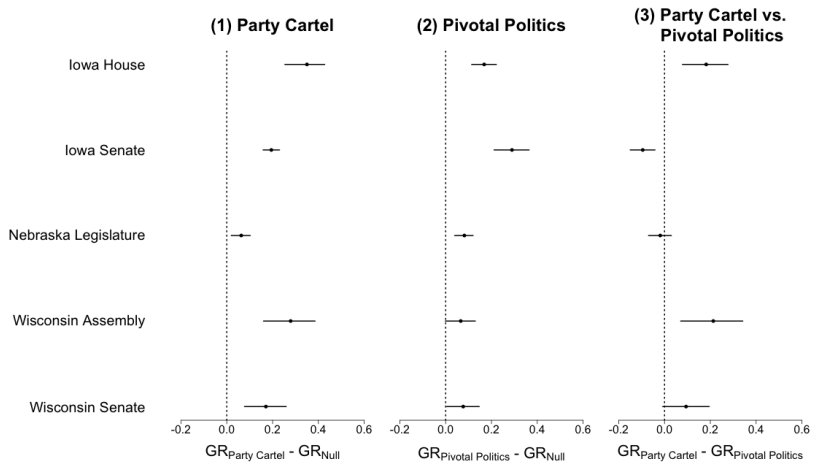
Appendix E.2: The Filibuster in Nebraska

In Nebraska, 33 out of 49 senators are required to invoke cloture (Rule 7, Sec. 10). Although floor debate is limited to 3×5 minutes for each non-introducing speaker per pending question (Rule 2, Sec. 10), each amendment is interpreted as a separate question, providing many opportunities to delay debate. Further, Rule 7, Sec. 11, which enables the Speaker to declare amendments/motions to be dilatory and therefore out of order, can only be invoked if a senator introduces more than 2 motions/amendments. Therefore, if 17 (49-32) senators participate in a filibuster with 2 amendments/motions each, with each participating senator debating 2 amendments/motions, the time allowed for debate exceeds 140 hours ($17 \times 2 \times 17 \times 15 = 8670$ minutes). In practice, the rule on dilatory motions does not get invoked, and open filibustering occurs.[74]

[74]See, e.g. the floor debates of Sen. Smith on LR 23 (02/23/04); Sen. Christensen on LB485 (04/03/14 and 04/04/14); and Sen. Chambers on amending Rule 7, Sec. 3 of the Rules (01/15/15). Transcripts are available at http://nebraskalegislature.gov/transcripts/search_past.php.

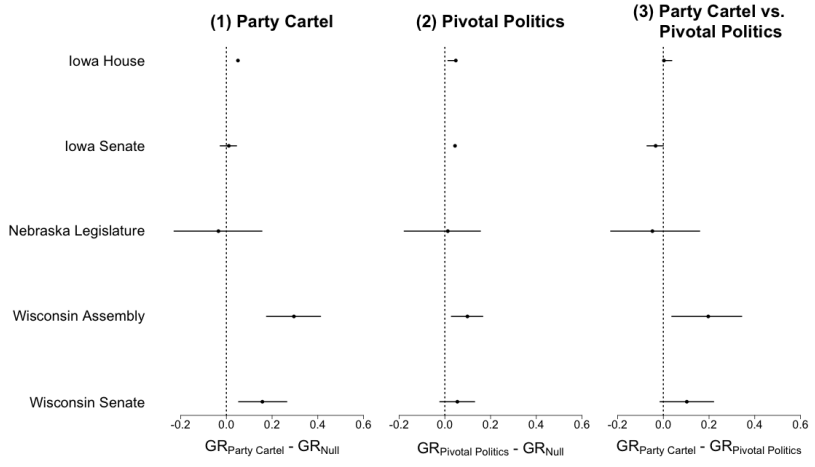
Appendix F.1: Gatekeeping Hypothesis Tests with Alternative SQs

Figure 2: Gatekeeping Hypothesis Tests with Unadjusted Vote-Based Status Quo Estimates



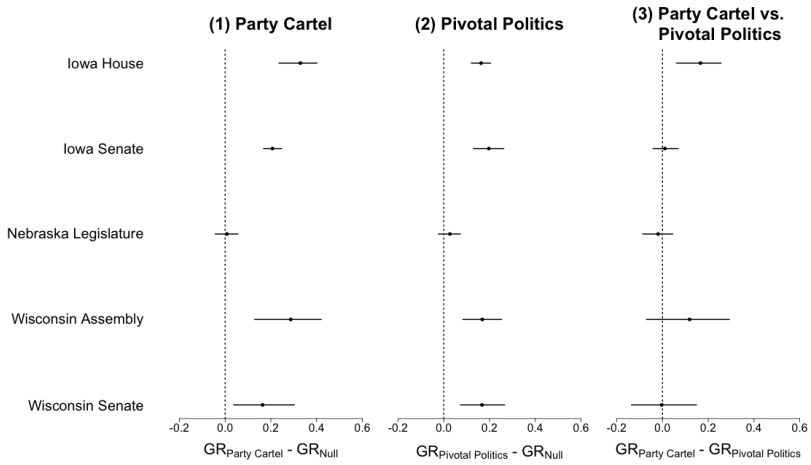
Note: The panels in this figure present hypothesis tests of nonrandom gatekeeping using the 95%-credible interval of the difference between two proportions. The first panel tests the party cartel hypothesis against a plausible null hypothesis: the proportion of estimated bills that are blocked. The second panel shows tests of the pivotal politics gatekeeping hypothesis against the plausible null. The third panel examines whether there is a difference between the performance of the party cartel and the pivotal politics gatekeeping hypotheses. Estimates rely on the assumption of homoskedasticity in σ.

Figure 3: Gatekeeping Hypothesis Tests with Cosponsorship-Based Status Quo Estimates



Note: The panels in this figure present hypothesis tests of nonrandom gatekeeping using the 95%-credible interval of the difference between two proportions. The first panel tests the party cartel hypothesis against a plausible null hypothesis: the proportion of estimated bills that are blocked. The second panel shows tests of the pivotal politics gatekeeping hypothesis against the plausible null. The third panel examines whether there is a difference between the performance of the party cartel and the pivotal politics gatekeeping hypotheses. All estimates are based on the joint model of bill cosponsorship and votes.

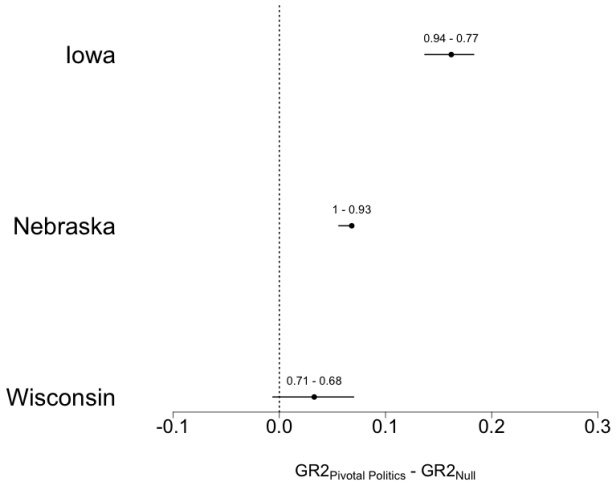
Figure 4: Gatekeeping Hypothesis Tests with Status Quos Based on Assumed Proposal Location



Note: The panels in this figure present hypothesis tests of nonrandom gatekeeping using the 95%-credible interval of the difference between two proportions. The first panel tests the party cartel hypothesis against a plausible null hypothesis: the proportion of estimated bills that are blocked. The second panel shows tests of the pivotal politics gatekeeping hypothesis against the plausible null. The third panel examines whether there is a difference between the performance of the party cartel and the pivotal politics gatekeeping hypotheses. Estimates rely on the assumption that the proposal location is given by the ideal point of the bill sponsor.

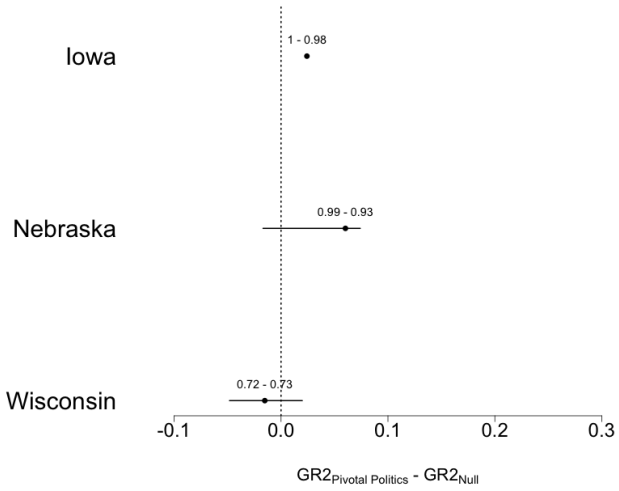
Appendix F.2: Gridlock Hypothesis Tests with Alternative SQs

Figure 5: Gridlock Hypothesis Tests with Unadjusted Vote-Based Status Quo Estimates



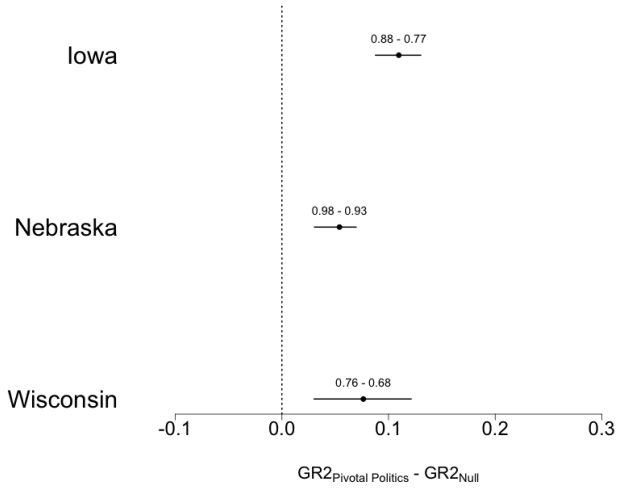
Note: This figures shows results from tests of the pivotal politics gridlock hypothesis against the null hypothesis of random bill passage in Iowa, Nebraska, and Wisconsin. The first number over the estimates is the gridlock ratio. The second number is the proportion of bills with estimates that are not enacted. Estimates are based on votes and the assumption of homoskedasticity in σ.

Figure 6: Gridlock Hypothesis Tests with Cosponsorship-Based Status Quo Estimates



Note: This figures shows results from tests of the pivotal politics gridlock hypothesis against the null hypothesis of random bill passage in Iowa, Nebraska, and Wisconsin. The first number over the estimates is the gridlock ratio. The second number is the proportion of bills with estimates that are not enacted. Estimates are based on the joint model of bill cosponsorship and votes.

Figure 7: Gridlock Hypothesis Tests Based on Assumed Proposal Location



Note: This figures shows results from tests of the pivotal politics gridlock hypothesis against the null hypothesis of random bill passage in Iowa, Nebraska, and Wisconsin. The first number over the estimates is the gridlock ratio. The second number is the proportion of bills with estimates that are not enacted. Estimates rely on the assumption that the proposal location is given by the ideal point of the bill sponsor.

References

Anzia, Sarah F., and Molly C. Jackman. 2013. "Legislative organization and the second face of power: Evidence from US State Legislatures." *Journal of Politics* 75(1): 210-224.

Carpenter, Bob, Andrew Gelman, Matthew D. Hoffman, Daniel Lee, Ben Goodrich, Michael Betancourt, Marcus Brubaker, Jiqiang Guo, Peter Li, and Allen Riddell. 2017. "Stan: A probabilistic programming language." *Journal of Statistical Software* 76(1). DOI 10.18637/jss.v076.i01.

Carroll, Royce, Jeffrey B. Lewis, James Lo, Keith T. Poole, and Howard Rosenthal. 2009. "Comparing NOMINATE and IDEAL: Points of difference and Monte Carlo tests." *Legislative Studies Quarterly* 34(4): 555-591.

Cox, Gary W., and Matthew D. McCubbins. 2005. *Setting the agenda: Responsible party government in the U.S. House of Representatives*. Cambridge: Cambridge University Press.

Crombez, Christophe, Tim Groseclose, and Keith Krehbiel. 2006. "Gatekeeping." *Journal of Politics* 68(2): 322-334.

Gelman, Andrew, and Donald B. Rubin. 1992. "Inference from iterative simulation using multiple sequences." *Statistical Science* 7(4): 457-511.

Hoffman, Matthew D., and Andrew Gelman. 2014. "The No-U-Turn sampler: Adaptively setting path lengths in Hamiltonian Monte Carlo." *Journal of Machine Learning Research* 15: 1351-1381.

Jackman, Simon. 2015. *pscl: Classes and methods for R developed in the Political Science Computational Laboratory, Stanford University*. Department of Political Science, Stanford University. Stanford, California. R package version 1.4.9. Available at http://pscl.stanford.edu/.

Jessee, Stephen A. 2016. "(How) can we estimate the ideology of citizens and political elites on the same scale?" *American Journal of Political Science* 60(4): 1108-1124.

Lo, James. 2013. "Voting present: Obama and the Illinois Senate 1999-2004." *SAGE Open* 3(4): 1-14.

Peress, Michael. 2013. "Estimating proposal and status quo locations using voting and cosponsorship data." *Journal of Politics* 75(3): 613-631.

Poole, Keith T., and Howard Rosenthal. 2007. *Ideology and Congress*. New Brunswick: Transaction Publishers.

Rosas, Guillermo, Yael Shomer, and Stephen R. Haptonstahl. 2015. "No news is news: Nonignorable nonresponse in roll-call data analysis." *American Journal of Political Science* 59(2): 511-528.

Stan Development Team. 2017. *RStan: the R interface to Stan*. R package version 2.16.2. Available at http://mc-stan.org.

Thieme, Sebastian. Forthcoming. "Moderation or Strategy? Political Giving by Corporations and Trade Groups." *Journal of Politics*. Available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3432733.